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CONTRIBUTIONS OF CHRISTIANITY TO BUDDHISM.[1]

THE Buddhist religion had penetrated to the most extreme northwestern part of India about the middle of the third century B. C. There it developed in the direction which expressed itself most distinctively in the deification of the person of Buddha and in the transformation of the Nirvāna-concept into the idea of a beatified continuous existence; there too arose the most essential points which distinguish northern Buddhism from southern in doctrine and forms of worship. This development found a positive conclusion in the establishment of a new school which assumed the name Mahāyāna, "The Great Vehicle," and which flourished in that region until about the eighth century A. D. After the founding of that school the older original Buddhism in contrast to it was called Hinayāna, "The Small Vehicle."

Expositions of Buddhism usually treat the Mahāyāna disparagingly, first because it places value upon the externalities of worship, and in the second place because in its philosophical speculation it evinces the strongest skepticism in teaching that Nothing is the true essence of things. But more important than these aspects of the varied contents of the Mahāyāna is the new ideal of life with which it has replaced the benevolent but fundamentally egoistic indifference—freedom not only from passions but even from all human emotions. This new ideal, which the early

[1] Authorized translation from the German by Lydia G. Robinson.

Buddhist type of saint no longer satisfied, was that of loving devotion and active compassion. H. Kern[2] says truly: "It is by that feeling of fervent devotion, combined with the preaching of active compassion, that the creed has enlisted the sympathy of numerous millions of people and has become a factor in the history of mankind of much greater importance than orthodox Buddhism." Southern Buddhism, which remained true to the ancient ideal, possessed no such winning power.

Moreover, the Mahāyāna exhibits ideas pleasing to the heart and imagination which run directly counter to the doctrines of the Hināyāna. The old Buddhism acknowledges no soul persisting throughout the course of life and knows no God, for the national gods which it recognizes are transient beings held captive in Samsāra. In the Mahāyāna we find a belief both in a personal soul and in God, at least in a kind of God. In a paradise called Sukhāvati where a reflection of the earthly Buddha, Amitabha, "the one surrounded by immeasurable light," sits enthroned in godlike fashion, the souls of the pious are born again after death in the buds of lotus flowers gradually to grow in the blossoms according to their deserts; and resting upon the lotus leaves they hear the good law preached to them by Amitabha or sung by birds in the leafy trees.[3]

According to the traditional statement, repeated even by Pischel and Edv. Lehmann,[4] the Mahāyāna was founded by Nāgārjuna, whose activity we would place rather in the middle than in the second half of the second century after Christ. But this statement is not correct. Nāgārjuna, who as originator of the Mādhyamika sect introduced into Bud-

[2] *Manual of Indian Buddhism* (Grundriss der Indo-arischen Philologie und Altertumskunde, III, 8, Strassburg, 1896), p. 124.

[3] Teitaro Suzuki, *Outlines of Mahāyāna Buddhism*, London, 1907; H. Hackmann, *Buddhism as a Religion*, London, 1910, pp. 50 ff.; Max Müller, *Last Essays*, II, pp. 304, 305.

[4] Pischel, *Leben und Lehre des Buddha*. Leipsic, 1906, p. 108; 2d ed. by Lüders, p. 104. Lehmann, *Der Buddhismus*, p. 227.

dhism the doctrine of Nothing as the only reality, was indeed one of the most significant and influential exponents of the Mahāyāna[5] and presumably the organizer of that school; but its foundation, that is to say the first literary exposition of its doctrines, must be placed about sixty to seventy years earlier. This was the work of a man who has latterly engaged the attention of the most distinguished Indologues, namely the famous and versatile monk Ashvaghosha, an elder contemporary of King Kanishka, hence in all probability living in the second half of the first century after Christ.[6] Ashvaghosha was an old man at the time of the birth of Nāgārjuna, that is to say, when the last Buddhist council was held at Jālandhara under King Kanishka about 100 A. D., if we may take as a basis of calculation the most probable but not quite assured dating of King Kanishka (last quarter of the first and the beginning of the second century). Cunningham, Pischel, the sinologue O. Franke, Fleet, and Lüders place Kanishka in the first century *before* Christ.

Therefore the appearance and the first propagation of the ideas of the Mahāyāna fall in the last decades before the council at Jālandhara.

It has occurred to many that Christian influences may have had some effect in the transformation of the Buddhist religion into the Mahāyāna form. Thus the sinologue Samuel Beal[7] found "in Ashvaghosha's writings many

[5] H. Kern, *Manual*, 6, pp. 122, 127. Teitaro Suzuki, *Açvaghosha's Discourse on the Awakening of Faith in the Mahāyāna*; translated for the first time from the Chinese version. Chicago, 1900, p. 43.

[6] Besides his best known work, the Buddhacharita which is a poetical biography of Buddha, Ashvaghosha wrote a collection of didactic tales (Sūtrālamkāra) and theological works and was also a successful composer and musician. Lately too by a happy discovery of Heinrich Lüders he has been shown to be a dramatist (*Sitzungsberichte der K. Preussischen Akademie der Wissenschaften*, phil. hist. Klasse, 1911, pp. 388 ff., especially 399; cf. also M. Anesaki, *Encyclopaedia of Religion and Ethics*, II, pp. 159, 160; S. Lévi, "Açvaghosha, le Sūtrālamkāra et ses sources," *Journ. As. S.*, Series X, Vol. XII, pp. 57 ff.

[7] *Abstract of Four Lectures on Buddhist Literature in China*. London, 1882, p. 95.

allusions and illustrations derived apparently from foreign, and perhaps Christian, sources," and arrived at the view "that much in the Buddhist development coming under the name of the Greater Vehicle may be explained on this ground." In another passage[8] he speaks in a more decided tone of the intercommunication in those days between East and West that "shaped the later school of Buddhism into a pseudo-Christian form."

A similar judgment has latterly been the fate of the oldest text-book of the Mahāyāna, Ashvaghosha's *Discourse on the Awakening of Faith in the Mahāyāna,* which is not preserved in the Sanskrit original but only in two Chinese translations.[9] The missionary Dr. Timothy Richard, who has translated this work into English (Shanghai, 1907), finds in it Christian ideas and influences and therefore reproduces the Buddhist terminology very freely in an entirely Christian mode of expression,[10] whereas on the other hand an earlier and more exact translator, the Japanese Teitaro Suzuki, a Buddhist (see above Note 4) has discovered no Christian traces of any kind in the book.

Lately, too, Christian influence in the Mahāyāna has been maintained by the Jesuit Joseph Dahlmann[11] with great determination and with an attempt at detailed scientific proofs. In what follows I shall first have to take his expositions into critical account.

In chapters 25-27 relating to the art of Gandhāra, that is of the Kabul valley and the surrounding country, Dahlmann has undertaken to show that these monuments of Buddhist art which reflect the Mahāyāna thought-cycle betray not only the generally recognized Greco-Roman

[8] *Op. cit.* Introduction, p. xiv.

[9] Bunyiu Nanjio, *Catalogue of the Chinese Translation of the Buddhist Tripitaka.* Oxford, 1883, No. 1249, 1250. There the title of the Sanskrit original is given as *Mahāyāna-shraddhotpāda*[*na*]*-shāstra.*

[10] *The Open Court,* XXV, 1911, pp. 251 ff

[11] *Indische Fahrten* (2 vols., Freiburg, 1908) II, pp. 100 ff.

influence but also a profound Christian influence. From the middle of the first century of the Christian era "that change in worship and art began to be consummated in Gandhāra. The same Buddha whose figure had been painstakingly avoided appears all at once in the monuments of Buddhist art, and not indeed as the simple herald of salvation as in ancient Buddhistic legend, but as in the message of salvation of *the Gospels, as God and as Saviour of the world.* He appears as God and Saviour not in Indian garments but in a garb *such as was worn by the higher classes in Antioch and Alexandria, in Jerusalem and Rome during the first centuries of the Roman empire.*"[12]

True and noteworthy, to be sure, is the circumstance that the likeness of Buddha appears first of all in the art of Gandhāra. Most investigators in Indian archeology have sought the reason for this strange fact and have found it in part (as in the case of Fergusson and Cunningham) in the assumption that the Buddhists had learned idolatry from the Greeks, whereas Grünwedel would fain explain the rise of the Buddha image from the natural development of Buddhism. In early Buddhist art as represented in the monuments of Sanchi, Bharhut and Buddhagayā in Central India, the original home of Buddhism, since the middle of the third century B. C., any likeness of Buddha is entirely absent. Where a likeness of Buddha would naturally be expected in the representations of his life and works we regularly find instead, in strange contrast to the lifelike pictures of all other participants in the scene, a symbol such as the tree of knowledge, a reliquary, or the wheel of the law. In the art of Gandhāra, on the other hand, the likeness of Buddha is the central figure. Here it appears everywhere in a commanding form even in the very same scene in which in ancient art it was replaced by a symbol. This likeness of Buddha

[12] *Op. cit.*, II, p.157.

passed with Buddhism from Gandhāra into all foreign countries which Buddhism conquered—into central Asia, China, Japan and the peninsula of Farther India.

That this surprising change which marks an epoch in Buddhist art can not be explained by external influences alone is obvious, although it must seem very natural that the artists of Gandhāra should rely upon the Greek types already known to them when they felt the need for the production of religious images. But these models would never have been able to accomplish this revolution alone. Such a change presupposes a transformation of Buddhist doctrine. In the original Buddhism Buddha was only a man who by his own power had found salvation from the sorrows of continuous existence and had shown the way by which it might be attained by everyone. Here there could be no worship; here the teaching was more important than the personality of the teacher, just as Buddha himself had said before his death in his last sermon: "The Doctrine and the Order which I have taught and proclaimed unto you — they are your master when I am gone."[13] The art of Gandhāra shows that the personality of Buddha had taken the place of the Doctrine and had become the object of worship. It is the visible witness of a transformation of fundamental views as it had advanced on the road towards the Mahāyāna.

Dahlmann's line of argument, however, places the greatest value on the rôle played in the Mahāyāna by the future Buddha, Maitreya. As we are later to criticise Dahlmann specially it will be better to give his standpoint in his own words. For this reason I have extracted a considerable passage from his work (II, pp. 127, 128):

"Many other Buddhas at long intervals had preceded Gotama Buddha in his calling as teacher of mankind. Gotama himself as the twenty-fifth was claimed to have com-

[13] Oldenberg, *Buddha*, 5th ed., p. 233.

pleted *forever* the series of the teachers of mankind. Therefore all hope of salvation was based on the doctrine he proclaimed. No other Buddha was to be expected in the future as teacher of salvation. To this idea a newly arisen school (the Mahāyāna) took exception, in so far as it supplied a successor as teacher of salvation to the Buddha now worshiped.... The Buddha Maitreya constituted the central point of this school. The earlier tradition knew nothing of Maitreya. As simple as it would have been to continue to spin the thread of the Buddhas reappearing at periodical intervals, yet the myth stood still at Gotama as the last Buddha. Buddha *Maitreya* in the form in which he is transmitted to us is a new creation.... But in the introduction of the Buddha Maitreya we have not merely to do with a new Buddha. Maitreya became the center of a new cult *in a character fundamentally different from the old Buddha,* and this character was *that of the loving compassionate Saviour who will one day come to liberate the world from the bonds of suffering.* Herewith there entered into this doctrine of salvation *an entirely new element in contradiction to the old tradition.* It directed the cult into the very path which the communities of monks had always resisted hitherto. The *teacher* becomes a *Saviour*; the *human being,* a *divine being* to whom man needs only to turn in trustfulness in order to be saved. In other words it is the *Saviour-idea* as incorporated in the Buddha Maitreya which called the Mahāyāna into existence."

That this conception of Dahlmann is in the main incorrect and easily disproved we shall see later on. At present we shall anticipate only one point. It must be granted that in the Mahāyāna a different character is assigned to the future Buddha Maitreya than formerly to the real Buddha, and that here indeed there exists a new element of which the old tradition knew nothing.

Dahlmann thinks that this new element can be explained only by foreign influence, and to him the only foreign influence worthy of consideration is that of Christianity.

The ardent joy with which Dahlmann proclaims this presumed discovery is easily understood, for in earlier works[14] he had tried to explain the fall of Buddhism in its own country by its intrinsic corruption. How well did this standpoint seem to agree with the knowledge, which Dahlmann thinks he has gained, that Buddhism does not owe its triumphal procession through central and eastern Asia and its dispersion over a third of all mankind to its own power but to Christian ideas by which it was enriched in northwestern India and attained its peculiar world-conquering vitality! Thus it would not be Buddhism which had subjected the peoples of eastern Asia but an offshoot of Christianity in Buddhist garb.

As comprehensible as Dahlmann's joy in his discovery is the enthusiastic applause which his thesis has received from some quarters of the Catholic press. Indeed, the positiveness of the assertion and the brilliant exposition in which Dahlmann has disposed of it seemed once for all "to have made an end of the Buddhism humbug." When we approach Dahlmann's spirited demonstration with a dispassionate critique it vanishes into nothing.

In the first place, what is the *chronological possibility* for the assumption that the appearance of the likeness of Buddha in the art of Gandhāra, the divinity of Buddha as attested by this art, and the conception of Maitreya as a divine Saviour can be explained by Christian influence? It is pretty well established that the art of Gandhāra reached its height at the end of the first and beginning of the second century A. D., but no time can as yet be definitely fixed upon for its beginning. The probability is

[14] *Nirvana, eine Studie zur Vorgeschichte des Buddhismus*, Berlin, 1896; *Buddha, ein Kulturbild des Ostens*, Berlin, 1898.

in favor of the pre-Christian period. The best specialists in this field, Grünwedel and Aurel Stein, have been inclined on account of the new discoveries in Turfan and Khotan to place the beginning of the Gandhāra art in the first or perhaps even in the second century before Christ.[15] And the first contemporary expert of Northern Buddhism, Louis De la Vallée Poussin, has practically settled[16] that the deification of Buddha in mythology and religion had taken place before the Christian era.

But if in spite of this we take Dahlmann's standpoint that the religion and art of Gandhāra originated in the Christian era we must further concede to him that Christianity had penetrated as early as the first century into the valleys of the Kabul and Indus—an assumption whose "possibility is not contested to-day in any quarter(!)"[17] Of course Dahlmann has to base this assumption upon a defense of the historicity of the St. Thomas legend because he needs the apostleship of St. Thomas in the Indo-Iranian territory for his demonstration.

Whereas earlier advocates of the historical character of the legend of St. Thomas, in so far as it relates to the Indo-Iranian territory, based their thesis upon discoveries of coins and one inscription by which the king in the Acts of St. Thomas, Guduphara-Gondaphares, was proved to belong to the first half of the first century after Christ, as well as upon reports of the international commercial relations of that day, Dahlmann brings forward the combination of apostleship and art in the person of St. Thomas as new and in his opinion the strongest evidence that the Christian influence in the art of Gandhāra could be explained through the activity of St. Thomas in India. This idea must be objected to on two grounds: (1) that Christian

[15] Wecker, *Tübinger Theol. Quartalschrift*, 92, note on p. 432.

[16] *Bouddhisme, Opinions sur l'histoire de la dogmatique,* Paris, 1909.

[17] Dahlmann, II, p. 138.

influence can not be proved in the art of Gandhāra; (2) that in the legend of St. Thomas, as O. Wecker justly remarks,[18] "the Christian apostle is not brought into relation with that kind of artistic activity which most clearly betrays the connection between Gandhāra and the west, that is to say with sculpture, but with the work of an architect and carpenter," which may probably be accounted for by the imagery of the construction of church or temple current in Christian modes of speech. Since I have given the reasons in this periodical (October 1911) why there can be no question of an historical nucleus in the Thomas legend, but that on the contrary Christianity did not penetrate into northwestern India at the earliest before the beginning of the third century, Dahlmann's theory becomes for us an historical impossibility.

But even a person who is not convinced of the unhistorical character of the legend of St. Thomas and who accordingly finds no difficulties in the question of chronology to prevent him from following Dahlmann's lead, can not be convinced by the arguments adduced by Dahlmann for Christian influence on the art and religion of Gandhāra, provided he understands how to pursue with the correct scientific method the beginnings of the development in early Buddhism which led to the later phenomenon of the Mahāyāna in dogma and worship. This has been shown very clearly by O. Wecker,[19] who nevertheless regards the historicity of the fundamental features of the Thomas legend as possible. To him everything that Dahlmann understands only on the assumption of Christian influence is to be accounted for quite spontaneously from the natural development of Buddhism. Some of his statements may follow here in his own words:[20] "In strange contrast to the *theoretical universality of the message of salvation,*

[18] *Tübinger Theol. Quartalschrift,* 92, note on p. 561.
[19] *Loc. cit.,* pp. 441 ff.
[20] Pages 442-444.

there stood from the beginning the difficulty with which the redeeming knowledge is to be gained, a difficulty so great that in fact the salvation of Buddha could never be a salvation for all, especially not for the many small and poor and weak.....As soon as the consequences were drawn from the universality of the salvation which Buddha preached, the exclusiveness of the pure Buddha doctrine must have been shattered; the postulates and ideas must necessarily be leveled and accommodated to the needs of every-day people as soon as the sermon becomes serious with its 'All ye, come unto me.' Is not this what happened? We need only point to the *transformation of the Nirvāna ideal*[21] to illustrate by a classical example the process of conversion which changed the pure teaching of Buddha into a popular religion....*A similar transformation of the person of Buddha was the natural consequence of this evolution.*"

The transformation of the Nirvāna concept, which moreover can not be explained solely by the change of the original doctrine of Buddha into a folk-religion, but

[21] When speaking in this essay of Nirvāna we mean salvation *after death.* Many discussions on the concept of Nirvāna suffer greatly from lack of clear-~~ness for~~ the reason that they do not take into consideration the ambiguity of the word Nirvāna, to which attention has been called first by Rhys Davids (*Buddhismus,* 118 ff.) and later by Pischel in an exhaustive argument (*Leben und Lehre des Buddha,* 2d ed., pp. 11 ff.). Even in ancient Buddhism the word Nirvāna was used not only in the sense of salvation proper which took place at the death of the Perfect One, that is in the sense of annihilation of existence, but also to denote *salvation during life,* that is the condition of complete rest and sinlessness which endures until death and is brought about by righteous living and redeeming knowledge. In distinction from this "salvation during life," which has also been a very current idea in the Brahman systems from pre-Buddhistic times until to-day, the real final salvation in death is for the sake of clearness often called Parinirvāna, "perfect Nirvāna"; but usually this distinction is not observed by the language in the texts.

I mention here this ambiguity in the Nirvāna concept because it continues also in the Mahāyāna. What Ashvaghosha says in his *Awakening of Faith* (Teitaro Suzuki, p. 87) about Nirvāna ("As ignorance is thus annihilated, the mind is no more disturbed so as to be subject to individuation. As the mind is no more disturbed the particularization of the surrounding world is annihilated. When in this wise the principle and the condition of defilement, their products, and the mental disturbances are all annihilated, it is said that we attain Nirvāna"), and what the translator (page 119 note) gives as the general conception of the Mahāyānists on the four stages of Nirvāna does not refer to the final Nirvāna but very distinctly to Nirvāna during life.

also by the progress of the doctrine among more active peoples filled with different desires and hopes, would according to Dahlmann's standpoint have to be referred to Christian influence, but strange to say Dahlmann has laid no stress upon the transformation of the Nirvāna ideal in his demonstration.

The deification of the person of Buddha becomes comprehensible from the natural evolution of Buddhistic doctrine not only by means of such general considerations as those we have just discussed. We can also[22] discover quite positive starting points for the path pursued in the alteration of the concept of Buddha. We must remember the charm exercised by the personality of Buddha upon his environment, and the reverence which was shown the master and which of course increased greatly after his death. Even in the formula of admission, "I take my refuge in Buddha, etc.," in the earliest period of Buddhism the person of the founder was placed before the doctrine. Then the worship of sacred places which played a rôle of particular importance in Buddha's life, and the worship of relics which started up in circles of the laity immediately after his death must have contributed to the exaltation of his person, as did also the formation of legends in which not only the life of the historical Buddha but also all the former existences ascribed to him were surrounded by the creations of an unchecked fancy. Even the monuments of early Buddhist art testify that the memory of the founder held the central place in religious thought; for although the likeness of Buddha was avoided (in order to give expression, as a matter of principle, to the thought that the doctrine is more important than the teacher) yet in reality all those old reliefs are "Buddha-centric."[23]

[22] With Wecker, pp. 445 ff.

[23] Wecker, p. 451.

Wecker is right however in laying most emphasis upon the *speculative and dogmatic development* of the old Buddhism. If the form of the one historical Buddha here becomes multiplied, further if beside those Buddhas (called in Sanskrit *pratyeka-*, in Pali *pacceka-*Buddhas) who are capable of attaining saving knowledge only for themselves but have not the ability to bring salvation to others, there appear the *samyak-* (Pali *sammā-*) *sambuddhas,* the holy universal Buddhas who appear at definite times in the various ages of the world in this and in other worlds with quite decided powers and signs in order to preach the saving knowledge, then already "Buddha's form in the belief of the Order had exceeded the limits of earthly human reality."[24] This elevation into the sphere of the supernatural may also have been favored by such stories as that of the conversation with the Brahman Dona[25] in which Buddha expressly states that men who have attained Buddhahood form a special category of beings different from gods, demigods and men.

With the multiplication of the historical Buddha there grew up the faith in future Buddhas for which there is evidence in the canonical Pali literature.[26] The dogma of the Buddha of the future is explained as readily as the deification of the historical Buddha from the evolution of the Buddhist religion. At the same time we do not deny that in the formation of the ideas of the future Buddha analogous foreign elements have cooperated. If the historical possibility and probability of such an influence must be admitted, it even becomes very credible. Dahlmann[27]

[24] Oldenberg, *Buddha,* 5th ed., p. 382; English translation by Wm. Hoey, p. 325.

[25] H. Kern, *Manual of Indian Buddhism* (Grundriss der indoarischen Philologie und Altertumskunde, II, 8) p. 64.

[26] In the Mahāparinibbānasutta, Dighanikāya, XVI, 1, 16, (in the Rhys Davids-Carpenter edition, II, p. 82) according to a kind communication from O. Franke.

[27] II, pp. 131-134.

takes action with great energy but with quite inadequate grounds against the theory that the Iranian ideas of the future Saviour, the Saoshyant (later Sōsiosh) could have influenced the thought-cycle of the Mahāyāna. And yet nothing is more obvious than this, since we are dealing with a time in which Iranian influences upon northwestern India have been plentifully established, as shown for instance on the coins of the Gandhāra period.[28]

Even in the Mahāyāna speculations on the five Dhyānibuddhas, the "Buddhas arisen from meditation," which are reflexes of the earthly Buddhas in transcendent worlds, the influence has been recognized of the Iranian doctrine of the Fravashis, those prototypes of all good creatures existing from eternity to eternity.

The main point against Dahlmann's theory, which brings the whole artificial structure to the ground at one stroke and which, strange to say, has been overlooked by Wecker, I have saved until the last. The foundation upon which Dahlmann's demonstration rests consists of the statement that the older tradition does not know anything at all of Maitreya, but that he is a new creation of the Mahāyāna. This assertion is also found elsewhere. Grünwedel[29] has the following to say about Maitreya: "The northern school is acquainted with him in full detail and puts revelations in his mouth; yes, he is everywhere highly venerated, almost more than Gautama. In the southern canon, as far as I can see, he does not appear, although the Singhalese chronicle Mahāvansa is acquainted with him."[30] Similarly we read in the supplemental volume of

[28] Wecker, *loc. cit.*, pp. 439, 440, 455. Grünwedel, ***Buddhistische Kunst***, 2d. ed., p. 167: "Hence we are perhaps justified in pointing out that here again contact with Iranian ideas has taken place. The similarity of the idea of the future Buddha Maitreya with the Saviour of the Pārsi religion Saoshyant (*Sōsiosh*) is very striking. Although we do not know when the legend of the Saoshyant as it now exists developed among the Iranians yet the dominant position of the Maitreya within the northern church has certainly been influenced by it."

[29] ***Buddhistische Kunst***, 2d ed., p. 158.

[30] Except the later continuations, it dates from the end of the fifth cen-

Brockhaus's *Konversationslexikon* (14th edition) in the article "Buddhismus," page *229a* on Maitreya (Pali, *Metteyya*): "The southern church acknowledges him but the canonical writings do not mention him. The Mahāyāna school which originated in the north betakes itself with peculiar zeal to the Maitreya cult and other Bodhisatvas." Of these two sentences only the second one is correct. A glance into the best known work on Buddhism[31] shows that the idea of the future Buddha Metteyya was not unknown to ancient Buddhism. T. W. Rhys Davids also says expressly that this doctrine already forms part of the system of the Small Vehicle (Hinayāna).[32]

The passage cited by Oldenberg (*loc. cit.*) is taken from the Cakkavattisuttanta, a part of the Dighanikāya and hence belonging to the canonical Pali literature. It reads: "He will be the leader of a band of disciples containing hundreds of thousands as I now am the leader of a band of hundreds."[33]

Further, Metteyya is called the future Buddha in the Buddhavamsa (27. 19),[34] a short poetical biography of the twenty-four former Buddhas which belongs to the appendices of the Suttapitaka. According to the preceding verse Kakusandha, Konāgamana and Kassapa were enumerated as the three Buddhas preceding the historical Buddha in this "blessed eon" (*bhaddaka kappa*). Now to be sure, as the editor observes, the Buddhavamsa orig-

tury after Christ. (See the citations for Metteyya in Childers's *Dictionary of the Pali Language*). Metteyya is moreover mentioned also in the Milindapañha, p. 159, which probably belongs to the second century after Christ.

[31] Oldenberg, *Buddha*, 5th ed., p. 164, note; 384 note 1.

[32] *Der Buddhismus*, translated into German by A. Pfungst, Leipsic, p. 208.

[33] Dighanikāya, Sutta 26. Even a scholar so familiar with canonical Pali literature as Prof. O. Franke considers this passage above suspicion and declares it to be impossible that it could have been interpolated in post-Christian times. Compare further C. A. F. Rhys Davids's review of Carpenter's edition of the Dighanikāya, Vol. III, *Journal of the Royal Asiatic Society*, 1911, p. 557. Professor Oldenberg has kindly called my attention to part of the following passages.

[34] Page 67 of Morris's edition, Pali Text Society.

inally ended with verse 18, and hence the two following verses and the last song (28) would be a later addition; but verse 19 only contains expressly stated what was already implied in the eighteenth verse. For according to the Buddhist doctrine there are not four but five Buddhas in a *bhadda kappa* (Sanskrit, *bhadra kalpa*); hence the mention of such a *kappa* implies the expectation of the fifth Buddha.[35] The eons are divided into "void" (Sanskrit, *shūnya*; Pali, *suñña*) in which no Buddha appears, and "not-void" (Sanskrit, *ashūnya*; Pali, *asuñña*), that is, full periods in which there are one or more Buddhas. The not-void eons bear special names according to the number of the Buddhas which appear in them (from one to five).[36] A *bhadda kappa* with five Buddhas like the present one always comes only after a long interval.

We have no reason to doubt that this entire idea of the different kinds of eons and the "eons blessed" with five Buddhas belonged to Buddhism before its development into the Mahāyāna. And since the name Maitreya-Metteyya, which from what we have said must be old (belonging to about the fourth century before Christ), is derived from the Sanskrit *maitri* (Pali, *mettā*) "love," so we can conclude that even in olden times the idea of loving compassion was bound up with that of the future Buddha.

We see that there is hardly a question in the history of religion which can be decided with greater certainty than that raised by Dahlmann and decided without any doubt, according to his opinion, in the opposite sense. The Mahāyāna has arisen without any influence on the part of Christianity and has overcome the eastern Asiatic world *by its own power* in a mighty triumphal procession, and

[35] Oldenberg, *Buddha*, 5th ed., p. 384, Note 1; Köppen, *Die Religion des Buddha*, I, p. 315.

[36] Spence Hardy, *A Manual of Buddhism*, p. 8; Childers's *Dictionary of the Pali Language*, s. v. "Kappo," p. 186; Pischel, *Leben und Lehre des Buddha*, 2d ed., p. 94.

at the same time to be sure without shedding a drop of blood, solely by the power of conviction and example. How great an influence, lasting even down to the present day, the Mahāyāna has exerted on the higher spiritual development of China, we learn from the great sinologist J. J. M. de Groot who lived in China for years among Buddhist monks and who declared that the Buddhists were the only Chinese who possessed refinement of heart, and the only ones with whom one could discuss spiritual matters.[37]

If we now turn to the question whether *at a later date* the demonstrable contacts with Christianity have left appreciable traces on northern Buddhism, I am inclined to answer in the affirmative, although it is difficult to give a positive proof.

Before I enter upon the subject of the Buddhism of Tibet, which here comes mainly into consideration, I shall add an incidental remark.

To the best known writings of the Mahāyāna literature belong the "Lotus of the Good Law" and the biographies of Buddha called Lalitavistara and Mahāvastu, none of which can be placed before 200 A. D. Most of the parallels with the Gospel stories which have been met with in Buddhist literature are found in these three works[38] (and besides in the Pāli Nidānakathā, the introduction to the Jataka book, dating from the fifth century after Christ).

Nothing more can now be said about these parallels except that it is not impossible that they were borrowed from Christianity. When in the later Mahāyāna writings mention is made of Buddha as a fisherman who catches men like fishes, and this comparison has passed over into Chinese art in which Buddha is represented as a fisherman with rod and hook,[39] we cannot fail to recognize here a

[37] See Edv. Lehmann, *Der Buddhismus*, p. 256.

[38] *The Monist*, XXI, October 1911, p. 520.

[39] Paul Carus, *The Open Court*, June 1911, p. 357.

transference of the Christian symbol into the Buddhist world, because the catching of fish is an entirely un-Buddhistic act. The same is true of the typical representation of the mother with the child Buddha. That this goes back to Christian prototypes one glance at the "Buddhist Madonna" from Chinese Turkestan in the Ethnological Museum at Berlin, is sufficient to prove.[40]

For such transmissions the conditions of those days were particularly favorable. Kennedy mentions,[41] although without stating his source, that in the eighth century a Christian monk and a Bactrian Buddhist together composed a Christian-Buddhist text-book. The fact is that in Singan-fu, the ancient capital of China, the Nestorian missionary Adam, the "presbyter, chorepiscopus and papas of China"—called by the Chinese King Tsing, the "distinguished and pure one"—together with Prajña, a Buddhist from Kapisha in Northern India, translated into Chinese the Buddhist Shatpāramitāsūtra from the Uigurischen.[42] Through the famous Chinese-Syriac inscription of Singan-fu, written in the year 781 by the above mentioned Adam with the aid of other Nestorians, we further learn that at that time in a monastery in that vicinity Buddhist monks and Nestorian Christians were living and working together side by side in a spirit of comradeship.[43] Such friendly intercourse between Buddhists and Christians probably existed in many places in central Asia in those times.

Buddhism did not penetrate into the icy highland of

[40] See frontispiece in A. Foucher's *Beginnings of Buddhist Art and Other Essays on Indian and Central Asian Archæology,* translated by L. A. and W. F. Thomas. Paris, 1912.

[41] *Journal of the Royal Asiatic Society,* 1907, p. 481.

[42] Takakusu, T'oung Tao VII, 1897, pp. 589-591; Berthold Laufer, *The Open Court,* August, 1911, pp. 451-452. According to this the emperor Tai-Tsung (780-804) distinctly issued a warning against the confusion of Christian and Buddhist doctrines.

[43] Max Müller *Last Essays,* I, p. 258; II, pp. 310 ff., according to James Legge, *Christianity in China,* 1888.

Tibet in the form of the Mahāyāna but of the Yogāchāra system,[44] which indeed wishes to be recognized as only a particular school of the Mahāyāna and which according to its own text-books is also called the Tantra school. This school was founded in the sixth century by the monk Aryāsanga of Peshawar, who adopted the Brahman—especially the Shivaitic—gods into Buddhism as defenders of the church against the world of demons, and furnished the religion with a confused theory of witchcraft in which predominated mystical formulas (*dhārani*) for the attainment of supernatural powers and the accomplishment of all possible desires.

In this degenerate form Buddhism reached Tibet in the middle of the seventh century,[45] and about a century later the church known under the name Lamaism, which soon developed into an ecclesiastical state, was founded by the artful "conjurer"" Padmasambhava whom the Indian missionaries of Buddhism called to Tibet from his native land Kafiristan in order to overcome the opposition of the native Shamans.[46] Padmasambhava succeeded in this conquest by incorporating the teachings and usages of these Shamans, who had great influence among the people, into Tibetan Buddhism in which since that time they have formed an important component part.

The possibility of Christian influence upon Buddhism in Tibet and China has existed since 635, for from this year we have evidence of a Nestorian mission which set out for those lands under a leader by the name of Olopan or Alopen.[47] This mission was received in northern India

[44] Literally, "practice of witchcraft," the chief characteristic of this school.

[45] Grünwedel, *"Der Lamaismus,"* p. 141, (In Hinneberg's *Kultur der Gegenwart,* Part I, Section III, 1: "Die Orientalischen Religionen." Berlin and Leipsic, 1906.)

[46] *Ibid.,* p. 143. L. Austine Waddell, *The Buddhism of Tibet or Lamaism,* London, 1895, pp. x, 24 ff.; see index.

[47] Waddell, *op. cit.,* p. 422.

by the famous king Shilāditya at his court in Kanoj in the year 639.[48]

Later there arose in Nepal and Tibet the belief in the Adibuddha, that is, in an omnipotent and omniscient primeval Buddha, who was supposed to have begotten the above-mentioned five Dhyānibuddhas by his meditation—hence a monotheistic transformation of the original atheistic Buddhism. Rhys Davids,[49] following Csoma de Körös, places the rise of this faith in the tenth century, L. de la Vallée Poussin[50] somewhat earlier. At any rate H. Kern and Waddell,[51] who rests upon his authority, are wrong in placing the beginnings of the doctrine of the Adibuddha as early as the first century after Christ.

Poussin regards this entirely theistic (*aishvarika*) Buddhism, which may be divided into several—at least into two—different Adibuddha systems, merely as a final stage in the evolution of the Mahāyāna. He says:[52] "Buddhist in fact only in name and in so far as it employs Buddhist terminology, it nevertheless is, as it were, the consummation of the philosophical, mystical and mythological speculations of the Great Vehicle, and differs from several other systems widespread in the Buddhist world, only by its markedly 'theistic' coloring." He mentions relations with Hinduism but never even alludes to the possibility of Christian influence. We shall have to concede to him that to insert a personal God, inactive in principle but in reality looked upon as creative—and as such we must consider Adibuddha—into the fantastic system of the later Mahā-

[48] Takakusu, I-Tsing XXVIII, note 8; *Athenaeum*, July 3, 1880, p. 8 in the review of Edkins's *Chinese Buddhism*; Grierson, *Encyclopaedia of Religion and Ethics*, II, p. 548 b.

[49] *Buddhismus*, p. 214.

[50] In the scholarly and exhaustive article "Adibuddha," *Enc. of. Rel. and Eth.*, I, pp. 93 ff., at the end of which is appended a comprehensive bibliography.

[51] *Buddhism of Tibet*, pp. 126, 130.

[52] *Loc. cit.*, p. 93 *b*.

yāna is quite comprehensible without foreign influence. As at first the imaginary Dhyānibuddhas and Dhyānibodhisattvas had been placed above the earthly Buddha and his many manifestations in the past and future, which had been accounted for as their earthly reflections, so later a basis might be sought from which those imaginary figures could be deduced, and this basis might be found in a supreme God. It is also conceivable that the desire to obtain adherents for the Buddhist religion among theistically inclined circles has contributed to the production of the Adibuddha. Poussin might have pointed out an analogous phenomenon in the history of Brahman philosophy, namely the introduction of the personal God (*ishvara*) into the atheistic Sāmkhya system, which in a less indirect manner was adopted in the formation of this system into the Yoga doctrine. Nevertheless it must be repeated that the conception of the Adibuddha may possibly be reducible to Christian influence since in Tibetan Buddhism religious discussions with Nestorians had undoubtedly preceded it in point of time.

With greater distinctness we can recognize the often alleged Christian influences on the later development of the Lamaistic form of worship which has been called a caricature of the Catholic service. Yet Catholic missionaries who had penetrated as far as Tibet have reported with horror that the devil had created a caricature of the ritual of the Roman Catholic church there in order to bring it into derision.

From Grünwedel's excellent exposition of Lamaism[53] we learn that the European Christian mission had exerted itself in behalf of Tibet ever since the first half of the fourteenth century. In the year 1330 Odoricus of Pordenone,

[53] In Hinneberg's *Kultur der Gegenwart,* Part I, Sec. III, I: "Die orientalischen Religionen," pp. 136 ff.; X, "Europäische Reisende in Tibet," pp. 156 ff. See also O. Wecker, *Lamaismus und Katholizismus, ein Vortrag.* Rottenburg, 1910; and Hackmann, *Buddhism as a Religion,* pp. 71 ff., 154 ff.

the first European who had succeeded in reaching the place, found Christian missionaries and some converts already in the capital of Tibet,—that is, in Lhasa. At any rate we must understand these missionaries to be Syrian Christians. In 1624 after a long interval the Portuguese Jesuit d'Andrada, coming from Delhi to the city of Chaprang in western Tibet, was received with honor by the ruling king and with his permission laid the corner-stone for a Christian church. We learn then of a series of other missionaries, Dominicans and Jesuits, from the beginning of the eighteenth century, of many hardships with which they had to contend, but also of protection and benevolence on the part of the king. In 1719 begins the missionary activity of the Capuchins, who had been successful at Rome in having the monopoly of the Tibetan mission conferred upon them. It was at once taken in charge by the Capuchins to a much greater extent. In the same year Horatio della Penna came to Tibet with twelve Capuchins, again in 1737 with nine, since most of his first companions had died or had become incapable of work. But towards the middle of the eighteenth century, soon after the death of Horatio, the Capuchins gave up the evangelization of Tibet.

We have no knowledge of any success their exertions may have had. If they had made converts to any considerable extent, surely all accounts of them could not have been so lost as to leave no trace. The missionaries were apparently wise enough to judge the matter correctly and to recognize the hopelessness of any considerable extension of Christianity in Tibet. But from the syncretistic character of Lamaism, which had adopted not only the Brahman gods but also the national divinities of the Tibetans and finally after the conversion of the Mongols even some of their ideas, they must also have been justified in

expecting there would be room within it for Christian ideas and Christian forms of worship as well.

With a similar view the Jesuits in China who had come in 1581 under the leadership of Ricci in the garb of Buddhist monks in order to secure a kindly reception, started out towards the end of the sixteenth century, and while publicly participating in Confucian worship diffused Christian ideas so that many Chinese accepted Christianity, but did not for that reason cease being Confucianists, Taoists or Buddhists, until finally a peremptory order from Rome put an end to this adjustment of Christianity to Chinese requirements.[54] So the Christian missionaries in Tibet would naturally have aimed upon the whole at the peaceful infiltration of Christian ideas into Lamaism in the hope of imperceptibly Christianizing it in time. That they succeeded better in this with regard to forms of worship than doctrine may be explained by the fact that Lamaism in contrast to the original Buddhism was directed essentially to externalities. In the high value placed upon sanctimonious observance Lamaism and Catholicism must have met on the same level.

In the year 1760 Tibet closed its doors to European visitors, and since that time only isolated Europeans—usually in the dress of Asiatics—have succeeded in penetrating into that country, but without reaching the capital Lhasa, with the exception of the British expedition under Colonel Younghusband, whose entry into Lhasa in the year 1904 is still fresh in our memories.

At any rate the seclusion of Tibet was complete when the two Lazarist fathers Huc and Gabet, in the garb of Buddhist ecclesiastics, arrived at Lhasa from Mongolia in January 1846 after a toilsome journey of a year and a half, and were compelled to leave again in March upon the demand of the Chinese Resident. The information which

[54] Max Müller, *Last Essays*, II, pp. 315-317.

Huc has given in his famous book[55] on Lamaistic forms of worship is an important source for all who have written on Lamaism.

Rhys Davids's *Buddhism* closes with the following comprehensive description: "Lamaism, indeed, with its shaven priests, its bells, and rosaries, its images, and holy water, and gorgeous dresses; its service with double choirs, and processions and creeds, and mystic rites, and incense, in which the laity are spectators only; its abbots and monks, and nuns of many grades; its worship of the double virgin, and of the saints and angels; its fasts, confessions and purgatory, its images, its idols and its pictures; its huge monasteries and its gorgeous cathedrals, its powerful hierarchy, its cardinals, its pope, bears outwardly at least, a strong resemblance to Romanism, in spite of the essential difference of its teachings and of its mode of thought."

This description could be further supplemented by reference to the crozier and the bishop's mitre, exorcism of demons, the censer with five chains which can be closed or opened at will, the benediction in which the Lama lays his right hand upon the head of the believer, the religious exercises in seclusion, and still other particulars.[56] Furthermore the practice of the higher Lamas to cross themselves before the beginning of a religious service[57] seems to me to deserve special mention, as does also a ceremony which bears a remarkable resemblance to the celebration of the Lord's Supper.[58] In this we have the distribution of consecrated bread and wine to the devout congregation. In place of the bread consecrated pellets of puff-paste are also mentioned, and by wine we must probably understand a

[55] *Souvenirs d'un voyage dans la Tartarie, le Tibet et la Chine,* 2 vols. Paris, 1850 (second edition, 1853); English edition, Chicago, Open Court Publishing Company.

[56] Huc in Wecker, *loc. cit.*, p. 37.

[57] Waddell, *Buddhism of Tibet,* p. 423.

[58] *Ibid.*, pp. 444 ff.

different sort of alcoholic drink.[59] At any rate "bread and wine" are enjoyed by the participants in this ceremony "for the attainment of long life." By long life may be understood a circumlocution for the Christian idea of eternal life.

One other fundamental idea of Lamaism appeals to us as strictly Catholic, namely that the priests "hold the keys of hell and heaven, for they have invented the common saying: 'Without a Lama in front (of the votary) there is no (approach to) God.'"[60]

One might be tempted to account for these correspondences between Catholic and Lamaistic worship as parallel phenomena by the statement that the human mind when moved in the same direction of thought and feeling arrives externally at the same results. But the correspondences are too close and too numerous for us to get along without the assumption of a loan. As at the close of my former essay in this periodical (October, 1911) on "Contributions of Buddhism to Christianity" I could not avoid the conviction that many fundamental features in the worship of the early Christian church have been taken over from Buddhism, so on the other hand at a more recent date many Christian forms of worship of a later stage of development have found acceptance in the most degenerate form of Buddhism, Lamaism.

I have pointed out above (pp. 182-183) how in my opinion this has come about. Huc has called attention to still another possibility.[61] In the thirteenth century in the times of the Mongolian supremacy, ambassadors from the rulers of the world came to Italy, Spain, France and England, and took home, so Huc thinks, a deep impression of the glitter and splendor of the Catholic worship. Per-

[59] "Ambrosia brewed from spirit or beer," Waddell, p. 445; in the middle of page 448 he speaks again of the sacred wine.

[60] *Ibid.*, pp. 422-423.

[61] In Wecker, *Lamaismus und Katholizismus*, pp. 37-39.

haps they did; but the incidental enthusiastic descriptions of these secular ambassadors would have assumed only very general outlines and could hardly have exercised any influence on the later worship of the Mongols. Still less propable is it that the Mongols would have carried traces of this influence to Tibet, since indeed they took Lamaism away from Tibet with them and have remained its true devotees until the present day. Moreover, at the time of their greatest power the Mongols, who were then adherents of Shamanism, were religiously indifferent, and ambassadors of Buddhism, of Islam and even of Christianity waited upon them in vain. When Kubilai Khan was converted to Buddhism in the thirteenth century the Mongolian empire had already fallen to pieces.

For the channels of Christian influence upon Lamaistic worship search must be made within Tibet itself, and at any rate the assumption of Huc[62] can not be ignored that the famous reformer of Lamaism Tsong-Kha-Pa (1356-1418), who introduced clerical vestments and a definitely prescribed ritual, had been under the influence of Christian missionaries, even though we possess no record from this period of a Catholic mission to Tibet. But central Asia was traversed in those days by numerous Christian missionaries, and so the "man from the west with the long nose and eyes gleaming with supernatural fire,"[63] with whom Tsong-Kha-Pa is said to have conversed, may have been a Christian monk who found his way there not from India (for then something more definite would be known of him) but from the north into the interior of Tibet.

At any rate since the Nestorians of the seventh century there have never been wanting channels through which Christian elements of worship might have been introduced into Tibetan Buddhism.

[62] See also Waddell, p. 59; Hackmann, *Buddhism as a Religion*, pp. 74, 75, 180.

[63] Huc, *Souvenirs*, II, 2d ed., p. 106.

In conclusion I should like to deny one possibility which has occasionally been suggested, namely that the Catholic ritual may not have influenced the Lamaistic, but *vice versa* may have been influenced by it.[64] Lamaism has never possessed the requisite strength for this. The side which is much weaker morally and intellectually can not urge its forms of life upon the stronger.

As we have seen, Christian influences upon the development of Buddhism are limited to secondary products of a late day; just as inversely Buddhist influences upon Christianity may be pointed out only in non-essential particulars and from times in which the doctrine of the Christian faith was established as a firm system. All identities and similarities in the teachings of these two great world-religions have, so far as *essential* matters are concerned, originated independently of one another, and therefore are of far greater significance for the science of religion than if they rested upon a loan.

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[64] Waddell, pp. 421-422: "It is still uncertain how much of the Lāmaist symbolism might have been borrowed from Roman Catholicism, or *vice versa*"; Pischel, *Leben und Lehre des Buddha*, p. 124: "Without doubt much has traveled from Lamaism into the Catholic church since even Buddha himself as Josaphat = Bodhisattva has been accepted among its saints in the Roman martyrology"—but not from Lamaism! We have distinct indications that Pischel is also the author of the (anonymous) article on Indian religions in Brockhaus's *Konversations-Lexikon*, 14th ed., XVII, where we read on page 594 b: "....so that the service of Lamaism closely resembles the Catholic service from which many would derive it....But the reverse path of the loan is equally probable."

THE PRINCIPLE OF RELATIVITY.

INTRODUCTORY.

PHYSICAL science seems to have entered into a new phase, the slogan of the new school being THE PRINCIPLE OF RELATIVITY. In some quarters the current modes of thought are declared antiquated, and the promise is made that the old truths will acquire a new meaning. Physicists speak of the relativity of time and space, and we will add that they ought as well speak of the relativity of things, of the whole actual world in all its parts and interrelations.

Many who have watched the origin and rise of the new movement are startled at the paradoxical statements which some prominent physicists have made, and it is remarkable that the most materialistic sciences, mechanics and physics, seem to surround us with a mist of mysticism. The old self-contradictory statements of the Eleatic school revive in a modernized form, and common sense is baffled in its attempt to understand how the same thing may be longer and shorter at the same time, how a clock will strike the hour later or sooner according to the point of view from which it is watched; and the answer of this most recent conception of physics to the question, How is this all possible? is based on the principle of the relativity of time and space.

The man who started this movement and was the first to formulate it in concise language and to base it upon close

argument was Professor Einstein,[1] who was followed by Lorentz,[2] and so we hear often of the Einstein-Lorentz theory. The strangest thing about it is that the question is seriously debated whether or not this theory is true, and the answer is expected from experiments; while in our opinion we are here confronted with a method, and the problem is simply how we can best deal with certain difficulties due to the relativity of all things. These difficulties have originated through the need of a greater exactness in measurements, but the underlying truth—the relativity of all things—is not a question of fact, but a recognition of certain complications with which we must learn to deal.

On reading recent expositions of the principle of relativity the man of good education, or the one who has attended universities without being a specialist in either mathematics or physics, feels the *terra firma* give way under his feet, and when he finds that the principle of identity seems to fail in his comprehension of things, a dizziness comes over his intellect and he sinks into the bottomless abyss of the incomprehensibility of existence. A general earthquake seems to quiver through his mind. Everything totters around him and he stands in awe at the significance of the new thought. Nor is there any one who dares to contradict; for the most learned arguments are adduced, the mathematical and logical conclusions of which bristle with formidable formulas,—yea, experiments are made to prove the truth of the relativity of time and space.

For the sake of convenience we will speak of the representatives of this new conception as the "relativity physicists" in contradistinction to the old-fashioned physicists of the old school. It has been said that the former represent more the mathematical aspect of physics while the

[1] *Jahrbuch der Radioaktivität und Elektronik*, 1905-1908.

[2] H. A. Lorentz, *Theory of Electrons* (Teubner) 1910.

latter are the realistic physicists proper, too realistic to understand the significance of the new truth.

In order to facilitate a comprehension of the situation as well as our own conception, we will here at once and dogmatically state that the relativity physicists are perfectly right; what they claim is really and truly a matter of course, and if they only would present their proposition without dressing up their theory in paradoxical statements, nobody would in the least hesitate to accept the new view. But as soon as this is done people will at the same time find out that the new view is not novel. Its importance has been greatly exaggerated, for the principle has been tacitly understood in the correct way by all preceding physicists who, at the time however, ignored, or better did not enter into, the problem, because they had other more pressing work on hand. Nor is it unlikely that they regarded this problem of relativity as a philosophical question which strictly speaking had no place before the forum of physics.

ON THE ABSOLUTE.

Perhaps the easiest way of elucidating the true meaning of the relativity of time and space will be by setting forth our own position as we held it long before the principle of relativity gained prominence or had even been mentioned or alluded to.

The writer's book *Fundamental Problems* contains the following statement under "Definitions and Explanations" (first edition, page 254; seecond edition, page 252):

"Absolute existence (in fact everything absolute) is impossible. Reality is properly called *Wirklichkeit* in German, derived from *wirken,* to take effect. Reality is not immovable and unchangeable absoluteness, but the effectiveness of things in their relations. Reality therefore implies not only existence, but the manifestation of existence

also. Existence and its manifestation are not two different things; both are one."

Since the days of Heraclitus it has been a trite truism that all existence is in a flux. There is no rest anywhere, and actuality consists in the effects which these changes exercise upon one another by action and reaction. Upon this lack of stability, resulting from a universal and intrinsic relativity, Mr. Spencer bases one of the strongest, though quite untenable, arguments of his agnosticism. He seems to expect that time, space, motion, and matter are or should be things-in-themselves, and forgets that they represent relations, i. e., certain features of reality. We will here quote his exposition of the unknowableness of motion in space. In his *First Principles* Spencer says:

"Here, for instance, is a ship which, for simplicity's sake, we will suppose to be anchored at the equator with her head to the west. When the captain walks from stem to stern, in what direction does he move? East, is the obvious answer,—an answer which for the moment may pass without criticism. But now the anchor is heaved, and the vessel sails to the west with a velocity equal to that at which the captain walks. In what direction does he now move when he goes from stem to stern? You cannot say east, for the vessel is carrying him as fast towards the west as he walks to the east; and you cannot say west for the converse reason. In respect to surrounding space he is stationary; though to all on board the ship he seems to be moving. But now are we quite sure of this conclusion? Is he really stationary? When we take into account the earth's motion round its axis, we find that instead of being stationary he is traveling at the rate of 1000 miles per hour to the east; so that neither the perception of one who looks at him, nor the inference of one who allows for the ship's motion, is anything like the truth. Nor indeed, on further consideration, shall we find this revised conclusion to be much better. For we have forgotten to allow for the earth's motion in its orbit. This being some 68,000 miles per hour it follows that, assuming the time to be midday, he is moving, not at the rate of 1000 miles per hour to the east, but at the rate of 67,000 miles per hour to the west. Nay, not even now have we discovered the true rate and the true direction of his movement. With the

earth's progress in its orbit, we have to join that of the whole solar system towards the constellation of Hercules; and when we do this, we perceive that he is moving neither east nor west, but in a line inclined to the plane of the ecliptic, and at a velocity greater or less (according to the time of the year) than that above named. To which let us add, that were the dynamic arrangements of our sidereal system fully known to us, we should probably discover the direction and rate of his actual movement to differ considerably even from these. How illusive are our ideas of motion, is thus made sufficiently manifest. That which seems moving proves to be stationary; that which seems stationary proves to be moving; while that which we conclude to be going rapidly in one direction, turns out to be going much more rapidly in the opposite direction. And so we are taught that what we are conscious of is not the real motion of any object, either in its rate or direction; but merely its motion as measured from an assigned position—either the position we ourselves occupy or some other."

The same argument of the captain walking the deck of a ship was made before Spencer, though mostly it was a ball rolling on deck; Bradley refers to it as well known in his time, 1727, and the same story has been repeated after Spencer. In fact it is one of the arguments of the relativity of space among modern relativity physicists.

The principle upon which the representatives of the new view take their stand is a consideration of actual life. Things are in a flux, and this is an undeniable fact. We must bear in mind that the way of making knowledge possible at all in the flux of being is to ignore what has nothing to do with the problem under investigation. Our method is based upon a fiction or, if you please, upon an artificial trick, viz., to ignore complications and to consider a certain thing as fixed; but there are cases in which we must remember that we ourselves change and that the very position we assume is moving.

This trick of assuming that our position is stable is easy enough because man does not at once notice that there is any change; but all things are in a flux and he himself

changes unconsciously. A primitive unsophisticated man does not know that the earth on which he stands is whirling around itself at the rate of 1037 miles an hour, on the equator, further that it is also revolving with incredible speed around the sun, and that with the sun it is proceeding in a spiral motion towards one of the constellations, probably the constellation Heracles, around an unknown center situated somewhere in the Milky Way. God only knows what else takes place and what kind of whirling dances the Milky Way performs. The savage has not the slightest idea of all this, and so it is easy for him to ignore the motion of which he unconsciously partakes.

If man really were aware of all the events which influence him, his head would swim, and he would be incapable of thinking any sober thought. Fortunately he is concerned solely with his own narrow interests. The more man in the further growth of his mind becomes familiar with these unnoticeable events, the more he discovers that for any particular purpose he must ignore what does not belong to the solution of the special problem under consideration.

This way of ignoring what does not concern us at the time is an artificial process, a process of abstraction and elimination, of cutting off all disturbing incidents, and in doing so the philosophically minded scientist will become aware of the fiction of arbitrarily laying down a point of reference which is treated as if it were stable while in fact, like everything else, it too is caught in the maelstrom of cosmic existence.

There is nothing wrong or harmful in this fiction; on the contrary it is an indispensable part of our method of comprehending things. The universe is too complicated to be understood or viewed at a glance, and knowledge, science, cognition as well as all mental processes become possible merely by concentration, i. e., by selecting a point of

view as being a certain fixed location from which we observe a change, an event, a transformation, in order to gain a comprehension of this or that piece of existence in contrast to others of the same or of a different kind. Such is the nature of cognition, and this artificial trick is an essential condition of observation.

Knowledge is relative. It is the relation between subject and object, the thinker and the thing, and this, far from being objectionable, is only the universal condition of all existence; for all existence is relative. All reality is the result of action and reaction; it is a forming and being formed under definite conditions; it is transformation. There is no existence in and by itself. Relativity is the principle of all real and actual being.

TRICKS OF COGNITION.

If the standpoint of an observer changes, the thing observed will naturally change too in its relation to him. Formerly physicists were in the habit of not seriously bearing in mind that the fixedness of their standpoint was an assumption; they did not follow this principle to its ultimate consequences. For their special problems it was not necessary to do so, and there is very little use in bearing it constantly in mind. The difference in time between the moment when the observer looks at an object and that in which the rays of light indispensable for observation strike his eye is too inconsiderable to be taken into account; it is a negligible quantity. But if the object under consideration is at such an enormous distance that it takes the rays of light thousands of years to reach the eye of the astronomer it does make a difference, and so James Bradley was astonished to register the fact that the fixed stars in the sky were not always in the same place but that they pendulated semi-annually above us with the motion of the earth around the sun. The direction in which we see them swings from

the aphelion to the perihelion, and a closer consideration of the facts shows that the rays of very distant stars which we catch in the aphelion are not caused at the moment when we see them but started thousands of years prior to the moment in which they strike the lens of the astronomer's telescope, and so the transference of rays of light from the star to the astronomer's eye at this enormous distance represents a relation which most forcibly drives the truth home to us that there is nothing absolute.

The same is true of all things. The object before us seems to stand there in a perfect and quiet completeness, and yet the changes that work unnoticed by our dull senses are constant, continuous and rapid. Heraclitus used to say that he could not come out of the same river into which he had stepped a moment before, because the water was always rushing by. Never is a drop of it the same, and this is true of all things, even of ourselves. The observer has to exclude from his methods of observation the fact that he himself, his senses and his mind, are in a constant flux.

In order to elucidate the significance of the nature of cognition as being a limitation and concentration upon one point and constructing artificial units, the writer has on former occasions used the analogy of the kinematoscope, the machine which produces moving pictures.

In order to make any picture possible we need a lens, and the lens focuses the rays of light so as to throw rays from the same spot upon one and the same place on the plane where the picture appears. The rays of light which proceed from an object scatter in all directions, and unless we use a lens to concentrate the rays, the formation of a picture of the object would remain impossible. Thus the method of producing a picture is by concentration.

The lens produces a picture by focusing rays of light, that is by throwing the same rays upon the same spot; but it would also be possible to produce a picture by cutting

off the redundant rays of light and singling out one or very few rays, each one coming from each of the several points of the object. Accordingly we can photograph objects through a pinhole; there is only this difference that the picture is weak and needs long exposure. This proves that the process of concentration is fundamentally a process of abstraction, of leaving out, of omitting the disturbing multiplicity of the innumerable facts of real life as represented in the totality of objective experience.

The kinematoscope involves not only the static form of things, their spatial expression, the juxtaposition of parts, but it also adds the changes that are taking place in time. The film of the kinematoscope consists of a series of pictures, one always a little different from another, and if these are presented in rapid succession the series is fused into one picture in which the succeeding differences appear as motion. This is accomplished by the introduction of a little winged wheel which in rapid succession covers and uncovers the several pictures. If we would take this little wheel with its wings out of the kinematoscope, and if otherwise the pictures on the film would succeed one another in a rapid continuous motion without this artificial separation by the wings of the wheel, we would see no picture at all but simply have a blur on the canvas. In order to have distinct pictures appear on the canvas, we must cut the flux of motion into little separate moments which we may allegorically characterize as atoms of time.

Reality is a continuous flux, but in order to follow it step by step we must do the same thing that the mathematician does with his differential calculus. In the calculus the curve is cut up into infinitesimal lines, which in continuous succession change their directions, and the smaller we conceive these lines to be, the less is the mistake made by this fiction, if they are treated like straight lines.

The method of the calculus, based upon the fiction of

substituting for a continuous curve a series of little straight lines constantly changing their direction, is not so very different from the method of cognition in general. Nor is there anything wrong in it, only we must remain conscious of the fiction. In a similar way we must know that existence itself is a continuous system of relations, or in other words, that relativity is the principle of all existence in the world of actual life as well as in the domain of thought. We must cut up the general flux according to the needs of our investigation and lay down artificial limits.

* * *

If we view the new physics under this aspect, it will lose its mystic glamor and at the same time appear intelligible. In fact we shall understand that the principle of relativity is a matter of course, and if we cut up reality into things, as if they were things-in-themselves, into units or atoms, we employ a trick of cognition which makes it possible to focus things and picture them distinctly in our mind.

There are large numbers of scientists possessed of an *odium philosophicum* because philosophy means to them some abstruse metaphysical system of thought which ignores the natural sciences and, spiderlike, spins a world-conception out of pure thought derived from the thinker's subjectivity. The result is that they are soon perplexed in their own science by philosophical problems; for true philosophy—the philosophy of science—is an indispensable factor of cognition, and its influence extends into the fabric of all scientific labors. Thus it happens that problems of a philosophical character arise unexpectedly, and then the information given by nature in reply to experiments is apt to be misunderstood.

If the reference point (R) from which an observer measures is in motion toward R_1, and the object observed (O) also possesses a motion of its own, we are confronted

with a complicated phenomenon. If R moves toward O, the object measured will be shorter than if it stands still, and it will be longer if R moves with O in the same direction. We have only to forget, after the fashion of the pragmatist, that there is an ideal of objective cognition, and assume that all there is about size or the objective measure of things consists in the result of our measuring and we have the clue to the paradoxes of the physics of relativity. If the point of reference is not stationary and if we neglect to account for its motion, the result of our measurement is necessarily vitiated thereby as much as the pragmatist's philosophy by his personal equation.

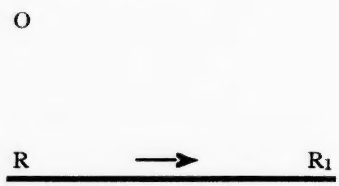


Fig. 1.

There are further complications of measurement. The time needed for the transmission of signals must also be taken into consideration. The rays of light travel at an enormous velocity but the distances in the starry heavens are also enormous and the distance between O and R is less than between O and R_1. The rays which were sent out from O at the moment of measurement have already passed the track of the observer at R, while this same observer has moved on to R_1, and there he catches the rays sent out from O in its position at O; in the meantime however the object O has in its turn also changed its place. From R_1 it appears at O, where it stood while the observer was stationed at R, but in fact it stands no longer at O but has in the meantime proceeded on its own path whithersoever that may have led O, backward or forward, in any

other direction than R, possibly in the same direction as R. Such phenomena are necessary results of the relativity of existence, and we must bear them in mind when confronted with complicated conditions which present themselves, for instance in astronomical cases. Here the mistakes rising from the fiction of assuming our reference point to be stable are considerable enough to enforce attention, and in that case we shall have to make allowance for the instability of our reference point, as well as for the time which the rays of light need for their travel through space.

That was exactly Bradley's case as set forth in his essay written in 1727, one hundred and eighty-five years ago, and thus he became the forerunner of the relativity physicists. To state it in other terms, Bradley correctly solved a problem which in our days led to the formulation of the principle of relativity, and he did so without mentioning this theory, yea without feeling the need of formulating it. He simply took it for granted that he had in this case to consider the motion of the earth that served him as a reference point—the place of his observations.

COMSTOCK ON RELATIVITY.

The most popular and at the same time the most exact characterization of the principle of relativity comes from the pen of Prof. D. F. Comstock, of the Massachusetts Institute of Technology. It appeared in *Science* (Vol. XXXI, 1909, p. 767), and we quote from it the passages which contain the statement of the problem:

Professor Comstock starts with the following two postulates:

> "The uniform translatory motion of any system can not be detected by an observer traveling with the system and making observations on it alone.
>
> "The velocity of light is independent of the relative velocity of the source of light and observer."

The main passages of his exposition state the problem thus:

"The whole principle of relativity may be based on an answer to the question: When are two events which happen at some distance from each other to be considered simultaneous? The answer, 'When they happen at the same time,' only shifts the problem. The question is, how can we make two events happen at the same time when there is a considerable distance between them.

"Most people will, I think, agree that one of the very best practical and simple ways would be to send a signal to each point from a point half-way between them. The velocity with which signals travel through space is of course the characteristic 'space velocity,' the velocity of light.

"Two clocks, one at A and the other at B, can therefore be set running in unison by means of a light signal sent to each from a place midway between them.

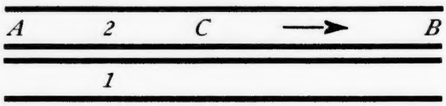


Fig. 2.

"Now suppose both clock A and clock B are on a kind of sidewalk or platform moving uniformly past us with velocity v. In Fig. 2 (2) is the moving platform and (1) is the fixed one, on which we consider ourselves placed. Since the observer on platform (2) is moving uniformly he can have no reason to consider himself moving at all, and he will use just the method we have indicated to set his two clocks A and B in unison. He will send a light flash from C, the point midway between A and B, and when this flash reaches the two clocks he will start them with the same reading.

"To us on the fixed platform, however, it will of course be evident that the clock B is really a little behind clock A, for, since the whole system is moving in the direction of the arrow, light will take longer to go from C to B than from C to A. Thus the clock on the moving platform which leads the other will be behind in time.

"Now it is very important to see that the two clocks are in unison for the observer moving with them (in the only sense in which the word 'unison' has any meaning for him), for if we adopt the first

postulate of relativity, there is no way in which he can know that he is moving. In other words, he has just as much fundamental right to consider himself stationary as we have to consider ourselves stationary, and therefore just as much right to apply the midway signal method to set his clocks in unison as we have in the setting of our 'stationary clocks.' 'Stationary,' is, therefore, a relative term and anything which we can say about the moving system dependent on its motion, can with absolutely equal right be said by the moving observer about our system.

"We are, therefore, forced to the conclusion that, unless we discard one of the two relativity postulates, the simultaneity of two distant events means a different thing to two different observers if they are moving with respect to each other."

We quote further:

"It must be emphasized that, because of the first fundamental postulate, there is no universal standard to be applied in settling such a difference of opinion. Neither the standpoint of the 'moving' observer nor our standpoint is wrong. The two merely represent two different sides of reality. Any one could ask: What is the 'true' length of a metal rod? Two observers working at different temperatures come to different conclusions as to the 'true length.' Both are right. It depends on what is meant by 'true.' Again, asking a question which might have been asked centuries ago, is a man walking toward the stern of an eastbound ship really moving west? We must answer 'That depends' and we must have knowledge of the questioner's view-point before we can answer yes or no."

The question of the man walking on a ship not only "might have been asked centuries ago," but it has been asked centuries ago. Our forebears were more conscious of the relativity of existence than the relativity physicists credit them.

Professor Comstock continues:

"It must be remembered that the results of the principle of relativity are as true and no truer than its postulates. If future experience bears out these postulates then the length of the body, even of a geometrical line, in fact the very meaning of 'length,' depends on the point of view, that is, on the relative motion of the observer and the object measured."

Professor Comstock's verdict of the case is summarized in this paragraph:

"The results of the principle for uniform translation are simply as true as its two postulates. If either of these postulates be proved false in the future, then the structure erected can not be true in its present form. The question is, therefore, an experimental one."

Here we demur. We claim that the question is not experimental but belongs to the department of *a priori* reasoning.

Professor Comstock does not enter into questions of mass connected with the principle of relativity but is satisfied with this comment:

"The apparent transverse mass is, I think, best derived by Lewis and Tolman,[3] in their excellent paper on the principle of relativity, and the relation between transverse and longitudinal mass is shown in the most direct and simple way by Bumstead[4] making use of the torsion pendulum. Any one interested in the subject should read these two papers."

THE A PRIORI.

It is characteristic of modern science to denounce the principle of the *a priori* and to extol experiment and experience. Now it is true that experience and experiment are indispensable factors in science, and in all the specialties of science. In experience and experiments we deal with the facts presented to us by nature; but the method of reasoning is not a thing which is derived from sense experience.

The method of reasoning is, as Kant truly said, *a priori* and, let us add, the *a priori* is nothing mystical or mysterious; it is simply the result of pure thought or reflection from which the data of the senses have been excluded. Pure thought (or better, purely formal thought) is a mental construction, or, if you prefer, a fiction. We omit every-

[3] *Phil. Mag.*, 18, 510-523, 1909.

[4] *Am. Jour. of Science*, 26, pp. 493-508, 1909.

thing concrete and thus we retain a field of abstract possibilities. Elsewhere we have called it a field of anyness.[5] Obliterating in our mind all particularity we retain nothing concrete and in this field of nothingness we build up pure relations. From this domain all real things, comprising everything which we subsume under the categories of matter and energy, has been excluded. But these pure relations, i. e., pure forms which are non-material constructions lacking all concrete qualities such as all real things possess, serve us as models for the relations of any possible purely mental or actual existence. Our doings in this field of abstraction consist in the fiction of pure lines, pure numbers, pure motion, pure ideas and their interrelations such as genera and species, and thus we are capable of building up a world of purely formal or relational thought, the totality of which in space is called geometry, and in the domain of numbers which originate by counting a series of single units, arithmetic, etc. In the domain of pure thought, consisting of genera and species, we call the laws that govern their relations logic, and the law of transformation, of which the positive aspect is properly called causality, and its negative counterpart the law of conservation of matter and energy, has been called by Kant pure natural science.

All systems of mental constructions have the advantage of picturing in our mind *any* possible configuration of relativity, and in this sense pure thought (Kant's *a priori*) is a field of anyness. It can be applied to any fact or set of facts of existence, actual or fictitious, and these systems of mental constructions therefore furnish us with the key to determine the relations of real nature. They render possible the systematization of sense impressions and thus

[5] See *Philosophy of Form*, the chapter on "The Foundation of Mathematics and Logic," pp. 7-10. For further details see also the chapter "Form and Formal Thought" in the author's *Fundamental Problems*, pp. 26-60.

these systems of pure thought in the field of anyness are the methods of scientific operation.

Let us not therefore speak contemptuously of the *a priori,* or denounce apriorism as something medieval and elusive, for even here in the attempt at establishing the principle of relativity in time and space, the arguments of the physicists are absolutely aprioristic. There is not one of these so-called experiments, invented to prove the relativity of time and space, which does not ultimately resolve itself into a machine that renders visible aprioristic considerations.

The ultimate arguments in all the experiments made to prove the relativity of time and space move in a domain of purely formal thought, and the force of them is ultimately of the same kind as the Q. E. D. of Euclidean theorems. We think here mainly of such propositions as locate an observer on the sun and another on the earth. Their clocks actually agree, but when compared they are found to differ. About eight minutes have elapsed when the observer on earth registers the time as the rays of the sun reach the earth, and *vice versa* when the clock on earth is observed as the rays from the earth strike the sun. The imitation of the same conditions for the sake of comparing the registration of two moving systems in an actual experiment amounts to nothing more than the pencil drawings of a Euclidean or logical figure in which the *a priori* reasoning is visibly presented as a *demonstratio ad oculos.* The argument remains in either case one of pure thought.

The photograph of such an apparatus built for the purpose of making an experiment in the relativity of time and space to show the difference between a solar clock and a terrestrial clock may be found in the article of Emil Cohn of Strassburg, "Physikalisches über Raum und Zeit" in *Himmel und Erde,* Vol. XXIII. To be sure the instrument does not fulfil the conditions either of distance or of

the velocity of the transference of the signal, "but," says Professor Cohn, "that is of secondary importance."

There are two motions both constant and both standing in a definite proportion. The sun with its clocks has been made to stand still. The earth with its two clocks moves, and there is an arrangement by which to represent the transference of signals. The main thing is that "their velocities stand in definite proportions and all that concerns us are these proportions. That we have here replaced the enormous velocity of light by a velocity of a few centimeters per seecond is unessential. It is essential, however, that the velocity of the earth is three-fourths the velocity of light, while the real ratio is 1 : 10,000."

Newton's laws are *a priori,* and Newton proves that these laws hold good in, and are serviceable as, interpretations of the actual world of fact. The empiricist ought to rebel against Newton's laws, because they never have been nor ever can be proved by either experience or experiment. Whoever saw a body moving in a straight line? and has Newton (from the standpoint of the empiricist) any right at all to make such sweeping statements of movements which have never occurred in the experience of anybody?

The most general principle at the bottom of scientific work is perhaps the so-called law of the conservation of matter and energy, and even this law is based on purely *a priori* arguments.

Incidentally we will say that the law does not hold good if we restrict the notion of matter to matter in the sense of the physicist which is mass, i. e., to concrete particles of existence that are extended and possess weight. It holds good only if we understand by matter the substance of being, its objective reality. We had better therefore speak of the conservation not of matter but of substance, for gross matter, consisting of the chemical elements, is constantly being produced before our eyes in the starry

heavens where the astronomers can watch the process through their telescopes. In the nebulas we see now the commotion of whirls with which gradually first the lighter and then the heavier chemical elements are being manufactured out of the original world-substance which we assume to be the same as the luminiferous ether.

Therefore we may surrender the law of conservation of gross matter, but we still hold to the conception that there is a conservation of stuff or substance, and the same is true of energy. There may be energy in the shape of a stress incorporated in the same wonderful world stuff, the ether, and this stress may be set free and become actual motion or kinetic energy, by some cause which creates those whirls that start the formation of nebulas.

And what proves the law of this conservation of substance and energy? It is the necessity of *a priori* thought which compels us to assume the principle that nothing originates from nothing and nothing disappears into nothing, which thought rests ultimately on the idea that all processes of existence are transformations. Everything that originates is formed by combination from something that existed before.

It has been maintained that the principle of relativity must be proved experimentally, but this is a mistake. Reality is everywhere a system of interrelations, yea every single concrete thing, every phenomenon, every piece of existence is a bundle of relations. It can be analyzed into its elements, which are actions and reactions; and that is all that reality means. Space as well as time are merely the measures, the former of arrangement or position, the latter of succession. Space denotes the interrelation of parts constituting figures or shapes affording a mode of determining direction and distance. Time measures the duration of events which is done by counting uniform cyclical motions or parts thereof. And so we must grant

that the relativity of time and space, as well as of all real things is a universal and inalienable condition of all existence. We can not think of any actuality which would not be dominated by relativity; which means we must regard the principle of relativity as an *a priori* postulate.

The principle of relativity is not established by experience but is ultimately based upon reflection and pure ratiocination. It belongs to the category of purely formal thought as much as all arithmetical and geometrical propositions.

If any proposition of purely formal thought, such as $2 \times 2 = 4$, does not hold good in our experience, we doubt the correctness of our counting or measuring, but we do not doubt our *a priori* proposition. We revise our observation, not our logic, our arithmetic, our mathematics; and suppose our observation proves true, suppose that 2×2 rabbits shut up in a cage are on recounting their number found to be more than four, say six or ten or any higher amount, we do not upset our arithmetic or any of our purely formal propositions, but seek the cause of the irregularity in the objects, in the things or animals counted. In that case we are positive that some transformation of the concrete material has set in which adds to the number to be expected according to arithmetical law.

If the reference point (R) belongs to the same system of motion as the object observed (O), our measurement will be correct and indicate the size of the object adequately. But if R moves in a direction and with a velocity of its own, different from O, the measurement will not be adequate; it will be warped in an exact proportion to the motion of R, and this rule holds good in the same way as all mathematical, logical and generally purely formal theorems.

The reliability of purely formal truths is not merely theoretical, but finds its application in practical life, in the

objective world of matter and motion, and can be verified by experience and experiment. And this is true also of the relativity of time and space.

If for instance a photographer takes the picture of a rapid express train in motion with a camera provided with a curtain shutter, the wheels will not be round but oval in the photograph, and the relativity photographer who identifies the picture with the thing, in the same way as the relativity physicist identifies the result of measuring with the objective size of the object measured, will claim that in proportion to the velocity of the train times the inverse proportion of the velocity of the slit in the curtain of the shutter, the wheels will increase their horizontal diameters and become that much more oval. Yea they will insist that the very same wheel will be at the same time in one camera, only a little more, in another one much more oval according to the quickness with which the slit of the curtain passes over the sensitive plate.

The relativity photographer will claim that the wheels in motion *are* oval while common mortals think that they only appear oval in the photograph.

Photographs do not lie; they show the objects photographed without any personal equation on the part of the photographer; their objectivity and impartiality can not be doubted, and here we see the wheels oval. They are oval, and their ovality, viz., their deviation from true circles, depends on the velocity of certain motions. An enthusiast for the principle of relativity can justly claim that *every photograph of a rapid train which shows the oval* form of the wheels is a successful experiment in the demonstration of the relativity of figure in space.

The truth of the principle of relativity in the domain of photography can be explained by *a priori* considerations. It is a matter of course, and if we argue the subject in our mind in pure reflection, we find out what we must expect,

and if finally we make the experiment, the principle proves true.

In the same way all the experiments made by machinery so constructed as to represent terrestrial and solar clocks or yard sticks, and to point out the unavoidable difference of measurements in both time and size resultant from their respective motions of the earth and the sun as well as the time it takes to transmit signals, are not experiments in the physicist's sense but expositions and demonstrations of purely formal truths which belong to the category of mathematics.

If the principle of relativity does not hold good in any domain of actual life, we must seek the cause in the material used and not in the principle of relativity. In other words we would be confronted with a purely physical problem which demands a physical solution, and this seems to be the case of the Fizeau experiment.

Prof. Emil Cohn, of Strassburg,[6] says:

> "It is strange that the relativity principle of mechanics does not hold good in radiation—in radiation and therewith in electrodynamics, for that the spread of radiation is an electrical process we may consider since Heinrich Hertz as an assured matter of experience. The decisive experiment which has been made by Fizeau is this: In a liquid, flowing with a uniform velocity, light is to be propagated in the direction of the current. According to the relativity principle an observer drifting in the current should find the velocity of propagation to be the same as if the liquid were at rest, and an outside observer should find the velocity of the light augmented by the full velocity of the current in the liquid. (Think, e. g., of the ball rolling on the deck of a ship in motion.) But such is not the case. There is added only a certain portion, viz., the index of refraction."

The very result of the experiment proves that one of the determinant factors is the physical property of the fluid.

When the principle of relativity is applied to positive

[6] *Loc. cit.*, p. 7.

facts we reach slippery ground, on which we must be on our guard to avoid mystification, for it would seem as if the law of the conservation of matter and energy were upset and all objectivity of scientific truth were lost. Experiments have been made to prove the principle of relativity with the result that Hupka and Bucherer,[7] the former with cathode rays, the latter with radium rays, demonstrate that mass increases with velocity as the relativity principle demands. Kaufmann, however, comes to the conclusion that there is an increase of mass but not as ought to be expected according to the principle of relativity, while Michelson and Morley demonstrate with great exactness that in spite of the motion of the earth the transmission of light is not changed at all, not within one hundred millionth of its proportion nor even a fraction thereof.

It would lead us too far to discuss the experiments made to apply the principle of relativity to physics and electrodynamics; we will only mention that (as *a priori* might be expected) they tend to corroborate its applicability in these domains.

ON ABSOLUTE MOTION.

Dr. Philipp Frank in his discussion "Does Absolute Motion Exist?"[8] declares that motion in physics always means "motion with reference to some definite body," and he recognizes that "this question is a philosophical one[9] but it is certainly not a physical question." The answer is the first Newtonian law, viz., "A body not affected by an exterior force moves in a straight line with a constant

[7] A. H. Bucherer, "Die experimentelle Bestätigung des Relativitätsprinzips" in *Annalen der Physik,* XXVIII, p. 513; "Messungen an Becquerelstrahlen" in *Physikalische Zeitschrift,* IX, pp. 755-760.

[8] "Gibt es eine absolute Bewegung?" Lecture delivered December 4, 1909, at the University of Vienna before the Philosophical Society. *Wissenschaftliche Beilage,* 1910.

[9] Dr. Frank adds here: "Perhaps the psychologist would call it a psychological one," but this would be a mistake. Psychology has nothing to do with the subject.

velocity which of course may be zero.[10] This is called the law of inertia."

If another force affects the moving body it is subject to the second law, the law of the parallelogram of forces, according to which the body will move along the diagonal of the two forces.

The following extracts translated from Dr. Frank's essay on absolute motion will prove instructive:

"The system of the fixed stars constitutes a fundamental body. Even in shooting a cannon ball towards the south we see no deviation from the law of inertia if we consider it with reference to the fixed stars. The ball remains in the same plane; but this plane does not retain the same relative position to the meridian of the earth, wherefore, of course, with reference to the earth the law of inertia is violated. On the whole it is evident that we really recover all the observed motor phenomena when we refer Newton's laws of motion to the fixed stars. Not until they are referred to the fixed stars do these laws acquire an exact sense which makes it possible to apply them to concrete conditions.

"We shall call those motions which are referred to a fundamental body 'true movements' and those related to any other body of reference 'apparent movements.' For instance the immobility of my chair is only apparent, for when referred to the fixed stars it is in motion.

"We now ask whether there are any other fundamental bodies aside from the system of the fixed stars. Obviously not any body revolving in an opposite direction to the fixed stars can be such a fundamental body, for considered with reference to such a body all rectilinear movements are curved. Therefore the law of inertia could not hold with reference to the body in question if it is valid with reference to the fixed stars. Then too a fundamental body can possess no acceleration with reference to the fixed stars, because otherwise there would be no uniformity of the motion of inertia with reference to it. However, these conditions are not only necessary but they are sufficient to characterize a fundamental body. All bodies moving uniformly and in a straight line with reference to the fixed stars will also be fundamental bodies inasmuch as rectilinearity and

[10] The original reads thus: "Corpus omne perseverare in statu suo quiescendi vel movendi uniformiter in directum nisi quatenus a viribus impressis cogitur statum illum mutare."

uniformity continue to hold for them, as do likewise the supplementary velocities determined by the second law. Accordingly Newton's laws do not indicate one single fundamental body, but an infinite number moving in opposite directions with a uniform and rectilinear motion.

"Hence we may well speak of 'true' in contrast to apparent rotary motion; for all bodies revolving with reference to a fundamental body revolve with reference to all other bodies. The same is true of true acceleration because an acceleration with respect to a fundamental body is also acceleration (i. e., change of velocity) with respect to all the rest. On the other hand, there is no sense in speaking of 'true' uniform rectilinear motion; for if a body possesses a uniform velocity with respect to the fixed stars, it is itself a fundamental body possessing of course with respect to itself a velocity of zero; it is at rest.

"Accordingly there is true acceleration, but not true velocity. From this is easily derived a proposition established by Newton which is called the principle of relativity of mechanics, namely that a uniform rectilinear movement of the system as a whole makes no change in the processes within the system; that is to say, we can not tell from the processes within the system what velocity the uniform rectilinear movement possesses with reference to the fixed stars. On the other hand, the rotary motion of a system has indeed an influence on the processes within the system, as for instance in the phenomena of centrifugal force; thus the earth has become flattened at its poles because of its rotation, or if I revolve a dish full of water the water will rise at the sides."

ABSOLUTE SPACE.

If we make measurements of motions which are limited to terrestrial conditions, the earth is and must be the system which, though not absolute, must for the nonce be so considered, and in that case the earth is called the fundamental or inertial body, of our measurements. But in many purely terrestrial motions we observe in very precise and exact measurements, deviations which compel us to seek for another fundamental body.

This happens in the case of the Foucault pendulum experiments and may also be observed in a cannon ball which

if shot south along the meridian will at a great distance show a deviation toward the west. Such experiments point out that the entire system of the fixed stars ought to be regarded as the fundamental body which thus would represent to us absolute space. I say here on purpose "represent to us," not "be," because we are most probably in the same predicament as persons moving in a train to whom the train and its interrelations, so long as the train does not move in a curve, represent the fundamental body or absolute space, viz., the ultimate system of reference.

It stands to reason that bodies in translation (in which the entire system as a whole moves in the same direction with the same velocity and without any internal change even of its smallest particles) behave as if they were at rest, and so the motion of a straight line cannot be observed so long as the observer remains limited to his own system. Every deviation from a straight line, however, implies a retardation on the inner side of the curve, or, what means the same, an acceleration on the outside of the curved path of motion. Accordingly all rotations bear witness to the character of their motion as appears in the Foucault pendulum experiment and in the flattening of the earth at the poles. Since further the idea of a rectilinear motion is a mere *a priori* postulate which can never be realized in actual nature, we see that every motion that takes place anywhere is affected by the totality of the universe. We must assume that its existence (the existence indeed of every particular thing or the recurrence of any event) must be understood to be a part of the whole. It bears traces of all the influences of all masses, and of all forces of the rest of the world according to the way it is interrelated with its surrounding conditions.

The fixed stars have so far proved sufficient for our terrestrial needs to serve us as a fundamental body for

calculations of a mechanical nature; but here the problem of absolute space presents itself.

We know positively that though the fixed stars are practically a fundamental body to us for mechanical measurements, they are shifting about among themselves and no more constitute something absolute than does our own earth; and yet there has risen a controversy on this subject in which Ernst Mach applies the principle of relativity throughout the universe while Prof. Alois Höfler stands up for what he calls the absolutist theory. We will hear what Dr. Frank has to say on this point:

"Is it to a certain extent accidental, or is it essential, that the tatality of the fixed stars coincides with that fundamental body in relation to which the laws of Newton hold valid? Or to put it more clearly: If the fixed stars were set violently in motion among each other and hence could no longer constitute a fixed body of reference, would the mechanical processes on earth proceed exactly as they did before? For instance, would the Foucault pendulum move just as at present, even though it now turns with the fixed stars, whereas in that case it would not be quite clear which constellation's revolution it should join?

"Were everything to remain as of old the fundamental system of reference would not be determined by the fixed stars but would only accidentally coincide with them, and would in reality be some merely ideal or yet undiscovered body. In the other case all mechanical occurrences on earth would have to be completely altered to correspond with the promiscuous movements of the fixed stars.

"It is well known that this is the view held by Ernst Mach. It alone holds with consistent firmness to physical relativism, and it alone answers the second main question of physics in the relativistic sense.

"The opposite view is represented by Alois Höfler in his studies on the current philosophy of mechanics, and lately by G. Hamel, professor of mechanics at the technical high school of Brünn, in an essay which appeared in the annual report of the German mathematical society of 1909 on 'Space, Time and Energy as *a priori* Forms of Thought.'

"Before I enter upon the controversy itself I would like further

to elucidate Mach's view by carrying out its results somewhat farther. In his well-known essay on the *History and Root of the Principle of the Conservation of Energy*[11] Mach ascribes to the distant masses in space a direct influence on the motor phenomena of the earth which supplements the influence afforded by gravitation. Of course no effect of gravitation from the fixed stars upon the earth can be observed, yet in spite of this they influence, for instance, the plane of oscillation of the Foucault pendulum because in Mach's opinion it remains parallel to them.

"The question now arises according to what general law of nature this influence operates which does not, like gravity, produce accelerations but velocities instead. Obviously this influence must be a property belonging to every mass, for according to our present conception the fixed stars of course are precisely the same sort of masses as earthly bodies.

"However, experience teaches us that terrestrial masses have no more influence on the plane of oscillation of the Foucault pendulum than has the changing position of the moon, sun and planets; but on the other hand it is exactly the most distant masses, the fixed stars, which determine its plane of oscillation. Accordingly we must either assume that the effect is directly proportional to the distance of the masses (which would be very strange indeed) or simply assume that this effect is proportional to the effective masses and independent of the distance, whence the dominant influence of the more remote, as the far greater and more numerous, bodies would naturally follow, and Mach inclines to this latter view.

"Mach's view shows most clearly in his position with regard to Newton's famous bucket experiment. In this Newton intended to show that the centrifugal force produced by a revolving body is due not to its relative but to its absolute velocity of rotation. He suspended a bucket filled with water by a vertical cord, twisted the cord quite tightly and then let it untwist itself, in this way setting the bucket to revolve rapidly. At first the water did not rotate with the bucket and therefore the bucket had a velocity of rotation with reference to the water while in the meantime the surface of the water remained undisturbed. In time, however, friction caused the water to become so affected by the rotary motion that bucket and water revolved like one homogeneous mass whereby the centrifugal

[11] Second edition, Leipsic, 1909; English translation by P. E. B. Jourdain, Chicago, 1911.

force caused the water to rise at the sides of the bucket and the surface became concave.

"Hence it is evident that the centrifugal force reached its greatest strength at the moment when the relative motion of the water with respect to the bucket became zero; hence according to Newton this force can be produced only by the absolute rotary motion of the water.

"To this now Mach justly protests that only the relative rotation of the water with reference to the fixed stars is to be considered, for this system of the fixed stars and not the bucket is the fundamental body. And indeed at first the water was at rest with reference to the fixed stars, but at the close of the experiment it was revolving. The mass of the bucket compared to the mass of the fixed stars is an entirely negligible quantity, so that it does not depend in the least upon the rotation. But we can not know, adds Mach, how the experiment would turn out if the sides of the bucket were miles thick; and by this he apparently means so thick that their mass would be considerable even when compared with the mass of the system of fixed stars. Then indeed might the rotation of the bucket disturb the action of the fixed stars.

"Höfler protests, on the other hand, that a system which is symmetrical round its axis could not according to all our experience in mechanics produce by its rotation that sort of an effect on the water within it.

"This also is quite true. But the effect of the masses assumed by Mach is such that it can not be expressed in our ordinary experiences with mechanics except by means of the facts of the inertia of all motion with reference to the fixed stars. New conditions such as the rotation of an enormously thick bucket might give rise to new phenomena. If we agree with Mach's view that the rotation of the plane of the Foucault pendulum is directly produced by the masses of the fixed stars, we must likewise admit, in order to be consistent, that the relative rotation of the very thick bucket might give rise to similar effects with reference to the water, as the rotation of the system of the fixed stars with reference to the earth to the plane of oscillation.

"Höfler expresses his contention against Mach's thesis in the form of the following question: If in Galileo's time the sky had been clouded over and had never become clear again so that we would never have been able to have taken the stars into our calculation, would it then have been impossible to have established our

present mechanics solely by the aid of terrestrial experiments? By this question Höfler means to say that if the connection with the fixed stars were a constituent of the concept of uniform motion, we would never have been able in such an overclouded world to have established the law of inertia, for instance, whereas in reality it is clear that this would nevertheless have been possible.

"I will not dwell on the more psychological question as to whether or how easily this would have been possible, but will only consider now the logical construction of mechanics in such a darkened world on the hypothesis that easily or with difficulty in one way or another we would have attained to our present knowledge of mechanics.

"Let us for a moment imagine ourselves in such a world. Above our heads extends a uniform vault of uninterrupted gray or black. Were we to shoot projectiles toward the south we would see that they describe paths which are curved towards the west; if we started pendulums to vibrating we would see that they would revolve their planes of oscillation in mysterious periods—I say mysterious because we might perhaps be able to perceive the change of day and night as an alternation of light and darkness, but would not be able to refer it to the movements of celestial bodies. Perhaps at first we would surmise that the motion of the pendulum could be ascribed to optical influences. I would like to see placed in such a world one of the philosophers who regard the law of inertia as an *a priori* truth. In the face of these mysterious curvatures and deflections he would probably find no adherents and he would not know himself what to make of his own standpoint.

"Finally, let us assume, there arises a dauntless man, the Copernicus of this starless world, who says that all motions proceed spontaneously in a straight line, but that this straight line is not straight with reference to the earth but with respect to a purely ideal system of reference which turns in a direction opposite to that of the earth. The period of this rotation is supplied by the period of the Foucault pendulum.

"This man would of course deny physical relativism upon the earth, for in his opinion terrestrial processes would not depend only on the relative velocities of terrestrial bodies but on something else besides, viz., their velocities with respect to a purely ideal system of reference. Nevertheless, he would not introduce any nonphysical element because for the purpose of the physicist a purely ideal system of reference whose motion with respect to an em-

pirical system is known serves the same purpose as would the empirical system itself. This bold innovator might finally refer the words 'true rest' and 'true motion' to his ideal fundamental body and so ascribe true motion and only apparent rest to the earth, thus maintaining a mechanics which would coincide literally with that of ours to-day, except that no small luminous points would be seen sparkling in connection with the fundamental body.

"Hence we see that physical relativism is not a necessary tool of the physicist. Apart, perhaps, from the psychological improbability—of which, however, nothing more positive can be said—the possibility of the development here indicated is logically free from objections throughout, and therefore the same is also true of the possibility of a nonrelativistic physics.

"But I would like to strengthen the argument of Höfler even somewhat further. That is to say, I would ask whether the world in which we live is then really so essentially different from that fictitious one. Imagine the dark roof which conceals the sky placed somewhat higher so that there is room beneath it for the fixed stars, perhaps as the dark background which may be seen nightly in the starry sky. The whole difference then consists in the fact that not only the Foucault pendulum and similar appliances move with reference to the earth, but enormously greater masses as well—all the twinkling lights of the sky by which the thought of a fundamental body in motion with respect to the earth is psychologically greatly facilitated, but logically is not much changed. Now imagine the sky of this earlier dark world suddenly illuminated; then we would see that the fictitious system of reference is closely linked to enormous cosmic masses, and it would be easy enough to accept Mach's hypothesis that these masses condition the fundamental system....

"If a distinction must be drawn between the respective values of the conceptions of Mach and Höfler, it is as follows: Mach's view adds decidedly more to the observed facts; for that it retains physical relativism does not involve freedom from hypothesis, because at best this relativism is theory and not fact. Mach sets up, hypothetically of course, a new formal natural law with regard to the action of masses existing side by side with gravitation, affecting the experiment very materially but unable to raise any claim to the simplest description of actual conditions.

"The other view, which simply introduces the system of reference procured by observation of the terrestrial and celestial movements without asking whence all this is derived, represents the pres-

ent state of our knowledge most adequately without any arbitrary addendum but also without giving the spirit of inquiry any incentive to new experiments.

"It is the old contrast between the most exact and least hypothetical representation possible of the known science, and progressive inquiry after new things in more or less daring and fantastic hypotheses. But Mach in this case stands in the opposite camp as in most other cases where his repugnance to all hypothesis has made him a pioneer in the phenomenological direction....

"I therefore believe I have proved that we can grant the following: Physical phenomena do not depend only on the relative motion of bodies without at the same time admitting the possibility of the concept of an absolute motion in the philosophical sense."*

Strange that Mach, with his reluctance to introduce anything hypothetical except what is absolutely indispensable, should range on the side of the theorists, and after some reflection I believe that there may be a slight hitch in Dr. Frank's interpretation of Mach's view.

First I myself, from my own point of view, would refuse to call the principle of relativity an hypothesis; it is an *a priori* proposition, a theorem, or if you prefer, a postulate of pure thought which either holds good universally, or has no validity whatever. So far as I know, Mach has not discussed this side of the subject but he has instinctively acted upon this view, and I would say that there is a greater hypothetical element in the assumption that the theorem $2 \times 2 = 4$, or any other proposition of the same kind, holds good only for our earth but not for Mars and Venus, than to say that it holds good also for the fixed stars and in the possible worlds outside of our Milky Way. Accordingly, whatever Mach's personal opinion may be, I would regard the universal application of the principle of relativity as less complicated and more free from hypo-

* This last paragraph is printed in spaced letters which indicates the emphasis of the author, and so we print the text of his summary in the original. Dr. Frank says: "Die physikalischen Erscheinungen hängen nicht nur von der Relativbewegung der Körper ab, ohne doch damit die Möglichkeit des Begriffes einer absoluten Bewegung im philosophischen Sinne zuzugeben."

thetical elements than its limitation to a portion of the world.

I can not as yet make up my mind to believe that our system of the Milky Way which furnishes us the grand sight of the fixed stars is an ultimate possessing the characteristics of absolute space.

According to Kant the totality of the fixed stars which are thickest in the Milky Way forms a great system (the system of the Milky Way) and our sun as well as all the visible fixed stars belongs to it. Kant believes that this, our own universe, which in the Milky Way appears to us as an enormous ring but together with the totality of the fixed stars must resemble an oblate spheroid, is not the only cosmic system, but that there are other similar systems outside of it and that they too whirl on through the infinity of space, in company with our Milky Way system, around some center of their own; and this very center of many Milky Ways may partake of a motion the observation of which lies hopelessly beyond our ken. Accordingly the space conditions of the Milky Way may serve *us* as absolute space, but there is a probability that this space is not more absolute than are the space relations in a quick but quietly moving train to the passengers.

Another point where we feel justified in doubting Dr. Frank's exposition is the statement that Mach hypothetically assumes a new law of nature as to the efficacy of masses, besides the law of gravitation. The passage in Mach's writings to which Dr. Frank refers does not (in my opinion) suggest the idea of an additional law of nature according to which the distant fixed stars should exercise a mysterious influence on the Foucault pendulum. We will later on let Mach speak for himself. In our opinion it seems that it would be sufficient to ascribe the rotation of the pendulum to its inertia while the earth revolves round itself, and this takes place in the space in which the earth

has its motion, viz., the space of the Milky Way system. The pendulum remains in the plane of oscillation in which it started while the earth turns around underneath. If there are influences at work beyond the expanse of the space of the fixed stars in our Milky Way system, they must affect the totality of our system and would therefore be contained in its space conditions; acting with an unfailing constancy they could not be separated from the properties of our space and would scarcely be discoverable.

There seems to me no need of inventing a new force besides gravitation. The law of inertia seems to explain the Foucault pendulum experiment satisfactorily.

The fixed stars as a totality remain in their places (at least as far as concerns the experiment) and the plane in which the pendulum swings keeps its original direction; thus the apparent motions of both coincide. Their space relations (the space relations of the pendulum and of the fixed stars) are the same, and there is no need to assume the existence of any unknown force exercised by the fixed stars upon the pendulum.

ERNST MACH.

We will let Mach state his views in his own words:

"Obviously it does not matter whether we think of the earth as turning round on its axis, or at rest while the celestial bodies revolve round it. Geometrically these are exactly the same case of a relative rotation of the earth and of the celestial bodies with respect to one another. Only, the first representation is astronomically more convenient and simpler.

"But if we think of the earth at rest and the other celestial bodies revolving round it, there is no flattening of the earth, no Foucault's experiment, and so on—at least according to our usual conception of the law of inertia.

"Now, one can solve the difficulty in two ways: Either all motion is absolute, or our law of inertia is wrongly expressed. Neumann[12] preferred the first supposition, I, the second. The law of

[12] *Ueber die Principien der Galilei-Newton'schen Theorie.* Leipsic, 1870.

inertia must be so conceived that exactly the same thing results from the second supposition as from the first. By this it will be evident that, in its expression, regard must be paid to the masses of the universe.

"In ordinary terrestrial cases, it will answer our purposes quite well to reckon the direction and velocity with respect to the top of a tower or a corner of a room; in ordinary astronomical cases, one or other of the stars will suffice. But because we can also choose other corners of rooms, another pinnacle, or other stars, the view may easily arise that we do not need such a point at all from which to reckon. But this is a mistake; such a system of coordinates has a value only if it can be determined by means of bodies....

"If we wish to apply the law of inertia in an earthquake, the terrestrial points of reference would leave us in the lurch, and, convinced of their uselessness, we would grope after celestial ones. But, with these better ones, the same thing would happen as soon as the stars showed movements which were very noticeable. When the variations of the positions of the fixed stars with respect to one another cannot be disregarded, the laying down of a system of coordinates has reached an end. It ceases to be immaterial whether we take this or that star as point of reference; and we can no longer reduce these systems to one another. We ask for the first time which star we are to choose, and in this case easily see that the stars cannot be treated indifferently, but that because we can give preference to none, the influence of all must be taken into consideration.

"We can, in the application of the law of inertia, disregard any particular body, provided that we have enough other bodies which are fixed with respect to one another. If a tower falls, this does not matter to us; we have others. If Sirius alone, like a shooting star, shot through the heavens, it would not disturb us very much; other stars would be there. But what would become of the law of inertia if the whole of the heavens began to move and the stars swarmed in confusion? How would we apply it then? How would it have to be expressed then? We need not worry about one body as long as we have others enough. Only in the case of a shattering of the universe we learn that all bodies, each with its share, are of importance in the law of inertia....

"Yet another example: A free body, when acted upon by an instantaneous couple, moves so that its central ellipsoid with fixed center rolls without slipping on a tangent-plane parallel to the plane of the couple. This is a motion in consequence of inertia. Here the body

makes very strange motions with respect to the celestial bodies. Now, do we think that these bodies, without which one cannot describe the motion imagined, are without influence on this motion? Does not that to which one must appeal explicitly or implicitly when one wishes to describe a phenomenon belong to the most essential conditions, to the causal nexus of the phenomenon? The distant heavenly bodies have, in our example, no influence on the acceleration, but they have on the velocity."

Now follows the passage to which Dr. Frank obviously refers:

"Now, what share has every mass in the determination of direction and velocity in the law of inertia? No definite answer can be given to this by our experiences. We only know that the share of the nearest masses vanishes in comparison with that of the farthest. We would, then, be able completely to make out the facts known to us if, for example, we were to make the simple supposition that all bodies act in the way of determination proportionately to their masses and independently of the distance, or proportionately to the distance, and so on. Another expression would be: In so far as bodies are so distant from one another that they contribute no noticeable acceleration to one another, all distances vary proportionately to one another."

We do not here understand Mach to fall back on the assumption of a new kind of force, and if we must grant that the distant masses exercise a dominant influence while the influence of the nearest ones (of the earth, the moon, and the sun) vanishes, we would say that this is due to the constancy of the distant masses which, as it were, is an inherent and inalienable part of all mass in the entire system and may be said to characterize its space conditions.

In speaking of "space conditions" I am conscious of using a term which Mach would repudiate, for he claims that for a comprehension of the concatenation of events, the notions of time and space are redundant. He says (*loc. cit.* pp. 60-61):

"To say the least, it is superfluous in our consideration of causality to drag in time and space. Since we only recognize what we

call time and space by certain phenomena, spatial and temporal determinations are only determinations by means of other phenomena. If, for example, we express the positions of earthly bodies as functions of the time, that is to say, as functions of the earth's angle of rotation, we have simply determined the dependence of the positions of the earthly bodies on one another.

"The earth's angle of rotation is very ready to our hand, and thus we easily substitute it for other phenomena which are connected with it but less accessible to us; it is a kind of money which we spend to avoid the inconvenient trading with phenomena, so that the proverb "Time is money" has also here a meaning. We can eliminate time from every law of nature by putting in its place a phenomenon dependent on the earth's angle of rotation.

"The same holds of space. We know positions in space by the affection of our retina, or our optical or other measuring apparatus. And our x, y, z in the equations of physics are, indeed, nothing else than convenient names for these affections. Spatial determinations are, therefore, again determinations of phenomena by means of other phenomena.

"The present tendency of physics is to represent every phenomenon as a function of other phenomena and of certain spatial and temporal positions. If, now, we imagine the spatial and temporal positions replaced in the above manner, in the equations in question, we obtain simply every phenomenon as function of other phenomena.

"Thus the law of causality is sufficiently characterized by saying that it is the presupposition of the mutual dependence of phenomena. Certain idle questions, for example, whether the cause precedes or is simultaneous with the effect, then vanish by themselves."

We understand that Mach endeavors to eliminate the terms time and space, because he wishes to correct the common notion which regards space as a big box into which the world has been packed. Mach says:

"Space and time are not here conceived as independent entities, but as forms of the dependence of the phenomena on one another. I subscribe, then, to the principle of relativity, which is also firmly upheld in my *Mechanics* and *Wärmelehre*."[13]

We agree with Mach. There is no time in itself; there

[13] Cf. "Zeit und Raum physikalisch betrachtet," in *Erkenntnis und Irrtum*, Leipsic, 1905 (2d ed. 1906, pp. 434-448); See also *Space and Geometry*, pp. 94 ff.

is no space in itself. Nevertheless, Mach has given much attention to physical space and appreciates the important part which it plays not only in the formation of our space-conception, but also in the actual world, for every spot of space possesses physical qualities according to the particles of mass which are there aggregated. Mach says:

"Since the positions in space of the material parts can be recognized only by their states, we can also say that all the states of the material parts depend upon one another.

"The physical space which I have in mind—and which, at the same time, contains time in itself—is thus nothing other than dependence of phenomena on one another. A complete physics, which would know this fundamental dependence, would have no more need of special considerations of space and time, for these latter considerations would already be included in the former knowledge."

The same idea is expressed by Mach in his Essay "Ueber den Zeitsinn des Ohres:[14]

"Physics sets out to represent every phenomenon as a function of time. The motion of a pendulum serves as the measure of time. Thus, physics really expresses every phenomenon as a function of the length of the pendulum. We may remark that this also happens when forces, say, are represented as functions of the distance; for the conception of force (acceleration) already contains that of time. If one were to succeed in expressing every phenomenon—physical and psychical—as a function of the phenomenon of pendulum-motion, this would only prove that all phenomena are so connected that any one of them can be represented as a function of any other. Physically, then, time is the representability of any phenomenon as a function of any other one."

We do not deny the truth of Mach's view. Nevertheless time and space are very convenient terms denoting two categories of certain interrelations (he would call them interdependencies) in the flux of things. Popular terms mostly originate because there is a need of them, and it seems to me it would be wiser to correct the errors connected with them than to drop them. If we pursue the

[14] *Sitzb. der Wien. Akad.*, 1865. Compare *Conservation of Energy*, p. 90.

latter policy we shall find ourselves obliged to reinvent a new collective term for certain classes of relations which belong together and can not be identified with other relations. The space and time relations are radically different from those of a purely physical, chemical or psychological nature.

We need not fear to retain the old terms, space and time, if we only bear in mind that there is neither absolute space nor absolute time but that the words denote relations. It seems to me that when Kant speaks of the ideality of space and time and insists on their non-existence as objective beings (*Wesen* or *Wesenheiten*) he attempts to say the same as Mach who declares that they are not "independent entities."

The conclusion at which we arrive in considering the nature of time and of space, be it from our standpoint of philosophy or from Mach's physical point of view, may be expressed in one word, that their most obvious characteristic is relativity.

CONCLUSION.

Professor Mach says in one of his notes quoted above, "I subscribe then to the principle of relativity," and so do I. Indeed I go one step further. I consider relativity as an inherent quality of existence and so I adopt the principle of it not as a result of experience but on *a priori* grounds. The principle of relativity, however, is frequently stated by relativity physicists as if the old ideal of science in its objective significance had to be abandoned, as if physics had to be remodeled, and as if the proclamation of the principle of relativity indicated a new departure from our traditional methods. This is not so, and I must insist that the principle of relativity has always been subconsciously in the minds of scientists. Only it has lately

been forced upon the attention of physicists by the progress in astronomical measurements.

How helpful the emphasis recently laid upon the principle of relativity will prove remains to be seen. Its ardent adherents exhibit great zeal which in many directions seems to be misdirected, and it appears to me that in spite of the correctness of the underlying idea their hopes are greatly exaggerated. After a while when the opponents of the principle of relativity will understand that its truth is as much a matter of course as the truth of the law of conservation of matter and energy, the contentions about it will cease and the evolution of science will no longer show evidence of excitement but will continue in its old quiet way.

There is more philosophy in our science than the school of empiricists are inclined to believe. It is very desirable that in familiarizing themselves with philosophy, these scientists should not fall back on the old systems of a visionary absolute, but they should adopt the philosophy of science, the only philosophy which is not a mere ingenious dream, and possesses objective significance.

The philosophy of science is *the* philosophy. It is the indispensable introduction to the study of any science and furnishes the basis for scientific method as well as a general survey of the assured results of all the several sciences. If the philosophy of science had been better known, the principle of relativity had at once been rightly understood and the vagaries of many mystifying contentions would have been avoided.

* * *

The purpose of this article is to set forth in general outlines the truth and significance of the principle of relativity, not to present an exhaustive treatment of it in all its phases and applications. We must bear in mind that in dealing with the several innumerable problems of exist-

ence science introduces a method which possesses certain limitations due to conditions which originate through some fictions of an apparently arbitrary nature assumed for the sake of isolating the object of investigation and concentrating upon it our attention.

We must bear in mind that we behold an object by focusing our eyes upon it and that only thereby can we form a picture of the object. It is a fiction to behold an object as if it were a thing by itself and it is positively impossible to see anything as it is in all its relations and with all its changes, past, present and future. Nor would such a comprehension of the object in all its entirety be desirable, for in the omneity of its relations we would see the whole universe while the special feature which concerns us sinks into insignificance. The same is true of science. Each of the several sciences selects its own field of investigation and thus constitutes a definite domain of abstraction for the sake of concentrating all attention upon it. For mechanics and for the measurements of motion in space, we need a reference point which must be able to be considered stationary, and if that is not the case we must refer both the movable place of observation, viz., the reference point (R) and the object observed (O) to one common system, which could be treated as, or must so far as R and O are concerned, actually be, stable.

We conclude by repeating that there is nothing absolute; all real and actual existences, all concrete things and happenings are relative, and if there is any thing that in a certain sense deserves the name absolute it is the truth as described in our mental fictions, the laws of purely formal thought, the eternal uniformities of purely formal relations such as we know from mathematics and all the other purely formal sciences; but even they are absolute only in the sense of constituting an entire system the truth of which is absolute, viz., it stands aloof and is founded in it-

self as a world of necessary conclusions built up in the field of anyness to serve as models for any conditions in any world actual or imaginary. And this absolute, this system of mental construction is after all a system of relations.

The more we ponder on the nature of existence, the more we shall understand the sweeping significance of relativity.

P. C.

INVENTORS I HAVE MET.*

ANY one who has been a professor of physics in a large city for several decades, unless he has earned a reputation for the crudest and densest Philistinism, must have made the acquaintance of divers thinkers and inventors who have taken counsel with him in their perplexities—thinkers of all kinds, schooled and unschooled, sanguine and timid, those that solve problems and those that create them; thinkers, suspicious and confiding, ambitious and practical; inventors at any price, and inventors on occasion.

It is obvious that the number of actual or alleged inventors in this company is greater than that of silent studious, self-centered thinkers. Practical discomfort is felt more often and to a greater extent than the rarer purely intellectual discomfort which is the heritage of men on a higher spiritual plane. Many fruitless hours may be spent in such consultations, but many a bit of psychological illumination may be gained and many a glance into the embryology of technique and science. We may add right here that the unlettered, unschooled or wild thinkers and inventors are the most interesting and instructive.

* * *

One day a gentleman was announced who had something of importance to communicate to me. He told me that he had taken a narrow tube full of liquid, closed at the upper end and open below, from which of course

*** Translated from the German by Lydia G. Robinson.**

nothing could flow because of the pressure of air; then he gave it a charge of electricity, whereupon the liquid began at once to flow. From this he drew the rash conclusion that the electric charge removed the air pressure. I gave instructions that an appointment be made with this gentleman for a free hour in the afternoon in order to make the experiment. But since one can easily tell whether or not a man is undertaking something from a purely theoretical interest, I said to the attendant in the laboratory, "The gentleman probably thinks he can drive a railway train with the electrical machine." In the afternoon considerably before the appointed time the stranger put in an appearance. "Are you thinking of driving a railroad train?" the attendant asked him by way of filling the interval with conversation. Immediately and without losing another word the gentleman seized his hat and was gone forever. So I had guessed his purpose correctly, and had deprived him of the pleasure of taking me into his confidence in his alleged lucrative undertaking. Forty years have passed since then, and the man has probably calmed down in the meantime.

* * *

There are people who become greatly excited over every scientific novelty, whose imagination busies itself at once in a new field without any special participation on the part of their intelligence, and whose desire it is to make an invention or a discovery in this field at any cost. So after the discovery of the Foucault rotation of the pendulum's plane of oscillation many experiments were made known by which it was thought this rotation could be perceived in water standing in a cylindrical tub across whose surface coal-dust had been lightly strewn; or again in a horizontal disk suspended by a thread, or in a scale-beam similarly suspended.

But obviously these experiments are not sensible. For instance, if a horizontal disk is actually at rest with reference to the earth it has of course the component of rotation of the earth around the perpendicular corresponding to geographical latitude; therefore the disk can not henceforth alter its position with reference to its terrestrial surroundings. Under other circumstances, however, it has an angular velocity around the perpendicular due to some impulse, to a draft of air, or the thread's momentum of rotation, and hence has no connection whatever to the Foucault rotation. One young man could not accept these reflections at all but persisted in repeating the experiment thus described by which he gained the interest of an old gentleman who observed in them "sometimes" the genuine Foucault rotation.

To be sure, Professor Tumlirz has recently performed an experiment which, while externally similar to this, is *correct*. By this experiment the rotation of the earth can be imitated, if the utmost care is taken, by the direction of the current of water flowing axially out of a cylindrical vessel. Further details are to be found in an article by Tumlirz in the *Sitzungsberichte der Wiener Akademie*, Vol. 117, 1908. I happened to know the origin of the thought that gave rise to this invention. Tumlirz noticed that the water flowing somewhat unsymmetrically in a glass funnel assumed a swift rotation in the neck of the funnel so that it formed a whirl of air in the axis of the flowing jet. This put it in his mind to increase the slight angular velocity of the water at rest with reference to the earth, by contraction in the axis.

The above-mentioned imaginative young man also constructed a telephone by a static electrical charge, and this invention likewise proved a delusion. Experimenting within the space of one room he had heard his own voice both as transmitter and receiver at the same time. Very often an

illusory invention bears witness simply to the ardent hopes of its originator.

* * *

Another young man declared that the theories of Galileo with regard to falling bodies and projectiles which he had learned in school were false; that the projected stone forms an entirely different problem from the falling stone; that the stone that is thrown is carried through the air and in the projection gravity is overcome. To this man the Aristotelian distinction between the natural falling motion and the violent motion of throwing is still valid. The fusion of the two primitive ideas into a unified whole had not yet taken place in his understanding.

* * *

Such a reversion to the primitive condition of science is not an isolated one. We may therefore conclude that after a disturbing interruption of the development of civilization science would again pursue almost the same course of evolution it had previously followed, although this of course would not preclude minor accidental discrepancies. Science has also its own natural embryology which is revealed through epistemology. Once I received an inquiry from the United States about the hydrostatic paradox which after Archimedes has been explained by Stevinus and for the third time by Pascal. The American writer declared that he could not understand how the pressure at the bottom of a vessel could depend upon anything else than upon the weight of the liquid resting on the bottom. Of course this was a very natural idea. I now proceeded to expound to the gentleman that the pressure at the bottom can not depend on the weight of the liquid resting on the bottom, but only on that portion of the weight which must be lifted in lifting the bottom, not the whole vessel. This seems to have met with comprehension at once. The

ingenious and spontaneous complacency of this American was altogether charming and delightful to me. He answered me in English since he knew no other language. He lived in "Cosmopolis"—street and number were unnecessary, simply the name of the writer sufficed. Hence the place was probably not yet Cosmopolis, but for the time being perhaps an embryo of five or ten houses which had undertaken to become a cosmopolis.

Intercourse with born thinkers of this type is very agreeable to me. Thus I would love to have known that naive Chinaman who, pointing to the street-car in San Francisco, the propelling force of which seemed incomprehensible to him, said (as my colleague B. Brauner tells me), "No pushee, no pullee, but it runs—."

* * *

One day I had a visitor whose external appearance proclaimed him every inch a man conscious of successful achievement. Without any doubt he was also intelligent, a good observer who had used his own eyes and knew how to turn his observations to practical account. He belonged to the class of inventors on occasion who base their constructions on practical and local knowledge and not on the fancy that something must be invented whether or no. He certainly deserved the success of his great business which extended over all Europe. But what surprised me was that he manifested such high theoretical aims at the same time. He felt like the laboratory assistant of Faraday who performed experiments while the great man only delivered the superfluous lecture about them. How could this great lecture, called science, have many difficulties for one who was so successful in his practical life, for that is the proof of the sum? Then too his theory was not in the least without foundation, for it rested on independent observation, that is to say, on what is called the Leiden-

frost experiment. But while he ascribed to this one observation an unduly enormous significance, he questioned at the same time the Newton theory of gravitation and all other possible theories, or undertook to base them on different foundations. My word for it, his observation was good, but onesided and incomplete, and therefore inadequate for a foundation of his theories and would not bear much fruit. He had a strong desire to rush at once into print. "If you wish to do that, my dear sir, I advise you at least to publish anonymously or under a pseudonym. In case you are ridiculed you can then join heartily in the laugh without anxiety for your reputation." The sensible man followed this advice and was splendidly successful in his book selling, for there are plenty of imaginative people who take pleasure in crazy theories. "Wisdom and experience in one field," I said also in the course of our conversation, "do not protect us from folly in another. You are efficient in your specialty and we will suppose that I am in mine. Would we not both be astonished and confused if you for instance would come out to-morrow as an obstetrician and I the day after as a dentist? And yet no less schooling and experience are needed for the conquest of a scientific specialty."

* * *

Many people feel that nothing else so cramps and limits their imagination as certain principles in science which are held to be firmly established and which others are used to look upon as providing the most abundant aid. Such a principle for instance is that of the equality of action and reaction, and another is that of the impossibility of perpetual motion.

Once I was urgently invited to visit a man who wished to show me something very remarkable. When I arrived he first told me the following story. He said that he had

never doubted the principle of the equality of pressure and counter-pressure. But once he had heard a traveler tell of an animal in South America that sprang with agility from branch to branch without communicating the slightest motion to the branches either as it left one or reached the the other. This aroused his interest so greatly that he went at once to South America in order to observe this squirrel-like animal. Here he convinced himself that the law of the equality of pressure and counter-pressure did not hold good. Upon his return he succeeded in devising an arrangement with which by means of cords fastened to one and the same body a motor tendency was communicated to this body. He showed me a ruler in which a motor impulse would arise by means of threads crossed and stretched in various directions between swivels. As he held it in his hand he said, "Now I feel myself drawn over there towards the door," whereupon he proceeded to step in that direction. "If that is so, sir," answered I, "you will easily be able to convince every one of the fact, if you will let this ruler swim freely on the surface of water so that it can move in a definite direction without your personal intervention." This he promised to do. I now felt myself impelled toward the door and took my leave as I began to feel somewhat uncanny. It was really very disquieting to remain in a place where, because of the inequality of pressure and counter-pressure, a tied-up package or a well-screwed piece of furniture would be able spontaneously and independently to get up and travel and fly at my head. It is now about twenty years since I have heard anything of this wonderful experiment.

* * *

There was an old gentleman of whom I was very fond who took a great interest in the problem of perpetual motion. He held that an instance of it must eventually be

found because it was necessary for the progress of humanity. The most diverse hydraulic and mechanical constructions were undertaken. When they were complicated enough so that they could not be seen through he thought he had reached his goal, but each time was of course disillusioned. Since he was an educated man I gave him Huygens's *Horologium oscillatorium* to read in which these conditions are set forth very clearly and simply, but it made no permanent impression. Ever and again his imagination overcame his judgment and ever and again triumphed the

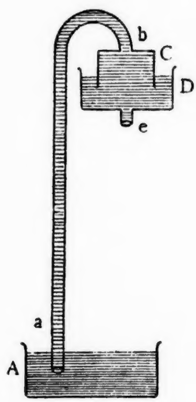


Fig. 1.

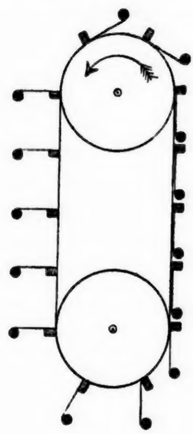


Fig. 2.

unshakable conviction of the necessity of this thing for the good of humanity. Somewhat similarly must Aristotle have thought with regard to the displacing of slave labor by the use of machinery.

One of the constructions of the old gentleman I remember very distinctly. It may be easily understood as presented in Fig. 1. A siphon *ab* dips into the vessel A and at the other end with a bell-shaped expansion C into the vessel D. If the openings *a* and *e* are left unobstructed then, according to the expectation of the inventor, the

small mass of water in the tube *ab* would follow the large masses of C and D and flow out at *e*. Instead of this, *Cba* behaved like a normal siphon flowing in the direction indicated by the letters, whereas a part of the water in D, to be sure, descended through *c* so that a break occurred between the water in C and in D, whereat the arrangement had failed to perform its function.

* * *

When I was a boy I had heard so much about perpetual motion that at a time when I had only a very superficial knowledge of the law of the lever I zealously set to work on the construction of a *perpetuum mobile*. The drawing in Fig. 2 will make clear the construction and its error. I was tempted to regard the horizontal bars with weights as somewhat long and efficient levers, although in this case there could be no question of levers and their rotation. Nature does not allow itself to be outwitted like the limited attention of man. To lift a weight P to the height H absolutely requires a weight P′ which reaches the depth H′, so that $P' \times H'$ is at least equal to $P \times H$. I can not say that this effort did me any harm. The mistake taught me to understand machines better than books or instruction could have done.[2] If any theory is of practical value in promoting civilization it is that of the limitation of available mechanical power, and no illusion is more harmful to progress than the idea of its inexhaustibility.[3]

* * *

One of the most remarkable inventors whom I have ever known was an old mechanic. At every detail he noted some advantage in construction and at once applied his idea. He reformed the handles and shape of beer glasses,

[2] The collection of constructions of perpetual motion machines preserved in the Technical Museum at Munich must be very instructive from a psychological point of view, as far as they can be deciphered.

[3] Indeed one of the greatest advances made in natural science rests upon the overthrow of this illusion through a fundamental employment of it.

laundry mangles, theater curtains; he constructed a clock from a barometer-tube closed at both ends in which a short column of mercury was placed at the side of a scale marked off empirically to measure time. He was a funny old fellow who wished to do away with the figures on the tower clock because "anyone would be a fool who would not be able to tell the time by the position of the hands." He was a born physicist. From his simple story I can not doubt that by blowing away the sawdust from a circular saw with perforations in the rim he discovered of his own accord the principle of the disk-shaped siren and the law of tone vibrations.

He was as extremely jealous of Cagniard Latour as if the latter by his much earlier observation had robbed him of the finest discovery. On the principle of the disk-siren he based his invention of a new musical instrument which he called a sirenophone. By means of a weight and a continuous cord a pedal set the system of the siren-disks in uniform rotation and at the same time worked a bellows. Piano keys, sunk more or less deeply with increased pressure, opened one or more tubes which blew with varying degrees of strength into the series of holes of the siren-disks so as to swell individual tones. The difference in pitch was obtained by the proportion of the radii of the pulleys over which the cords of the disks were drawn. This instrument made far more pleasant music than a harmonium and it would be simply impossible for it to get out of tune. It could be manufactured in perfect tune by a simple method of stamping. When a young man proposed to the inventor to sell his invention but keep its name, he received the answer, "The invention is great but unsalable." Hence he apparently preferred that it continue its existence as unique and legendary rather than be a source of profit. When a colleague once tried to play the instrument the inventor fell upon him furiously and declared it

was a sacrilege. The inventor surrounded himself with the mystery of a medieval wizard and conjurer. The orders of the minor petty German princes for whom he had arranged various theatrical details he wore with ostentation and listed them carefully upon his visiting cards. This man's vanity greatly diminished the impression of his very considerable talent and disturbed his relations with his hardly less gifted brother.

* * *

In my institute I once had a very gifted young man D. to whom I proposed that he carry on a piece of work in physiological optics in which he made good progress. One day I came to him with the question, "Well, what are you doing?" "Nothing," was the answer, "because I haven't any pasteboard to make a new disk." "Well, if that is all it takes to put a stop to your research you will not get very far," was my reply. This episode would not have remained in my memory if D. had not reminded me of it years later. But it is noteworthy that soon afterwards he completed a series of fine tasks for which he had provided himself with all necessary devices in the simplest way possible; he almost never had need for anything from the materials of the institute. He constructed a Jamin compensator by cutting a slightly curved optical lens. I must add that I have seen many similar accessories in the collection left by Nörrenberg in Tübingen. There stood whole cases full of the cleverest optical apparatus made out of cork and glass. Nörrenberg let the endowment lapse and made his apparatus himself in order not to have to write everything down in the inventory book and keep a strict account of it. Every curator of an institution is familiar with this burden which always intrudes upon his most convenient time for work, or on his vacation.

The young man D., who was the exact opposite of the

preceding one in seriousness and simplicity, soon became my assistant and left with me a cheerful memory of his dry humor. When I was demonstrating to beginners the interference bands of the sodium flame by the greater thickness of layers of air of the Newton glass and bade them not to focus their eyes upon the flame but on the glass, they did not all suceeed in this at once. With averted face the assistant scattered a few grains of salt over the glass, with the words, "There now, look at the salt!" When I pointed out the Talbot bands by covering half of the pupil with a piece of mica many looked through the mica and many looked past it. The assistant cut a small hole in a piece of black paste board and covered the half with mica, saying: "There now, look through the hole!" When I called attention to the range of oscillation of a string which vibrated the fundamental tone and the octave at the same time, one of the class was almost misled into considering it two strings. "Put your finger in between quick, then you will have two!" said the assistant.

* * *

In this brief review we have not drawn any sharp distinction between inventors and thinkers, between invention and discovery. Indeed there is no great difference. The liberation from a practical discomfort by a new procedure we call an invention. But if we feel an intellectual discomfort, in that for instance we can not follow in our thought an unaccustomed fact and can not see through it, then we call a serviceable guide of our thoughts which helps us to do so a discovery. When a man fiinds he can not boil water in a pumpkin shell because it catches fire he invents the pot by surrounding the pumpkin with clay. When a man can not understand the light and dark bands in conflicting rays of light from two identical sources because he thinks of light as a uniform stream he discovers inter-

ference from the instruction to represent light with periodically changing properties. Discoveries and inventions may be due to an accidental occasional observation, as is shown in the above examples. In other cases they may be the result of prolonged systematic work as has been illuminatingly presented by the Muscovite engineer P. K. v. Engelmeyer in his essay *Der Dreiakt als Lehre von der Technik und der Erfindung* (Berlin, Heymann, 1910).[4] If an invention is to be made there must be the *desire* to remove an inconvenience; there must be the *knowledge* of the means by which this can be done, and the *ability* to make a practical application of them. This is the *Dreiakt* of the *purpose*, the plan for *attaining* it and the material performance which takes place *mutatis mutandis* also whenever a theoretical problem is put to a practical application.

ERNST MACH.

VIENNA, AUSTRIA.

[4] See a further account of this work in the editorial in this number entitled "A New Theory of Invention."

THE NEW LOGICS.[1]

I. THE RUSSELL LOGIC.

TO justify its pretensions, logic had to change. We have seen new logics arise of which the most interesting is that of Russell. It seems he has nothing new to write about formal logic, as if Aristotle there had touched bottom. But the domain Russell attributes to logic is infinitely more extended than that of the classic logic, and he has put forth on the subject views which are original and at times well warranted.

First, Russell subordinates the logic of classes to that of propositions, while the logic of Aristotle was above all the logic of classes and took as its point of departure the relation of subject to predicate. The classic syllogism, "Socrates is a man," etc., gives place to the hypothetical syllogism: "If A is true, B is true; now if B is true, C is true," etc. And this is, I think, a most happy idea, because the classic syllogism is easy to carry back to the hypothetical syllogism, while the inverse transformation is not without difficulty.

And then this is not all. Russell's logic of propositions is the study of the laws of combination of the conjunctions *if, and, or,* and the negation *not.*

In adding here two other conjunctions *and* and *or,* Russell opens to logic a new field. The symbols *and, or* follow the same laws as the two signs $\times$ and $+$, that is

[1] Translated by George Bruce Halsted.

to say the commutative, associative and distributive laws. Thus *and* represents logical multiplication, while *or* represents logical addition. This also is very interesting.

Russell reaches the conclusion that any false proposition implies all other propositions true or false. M. Couturat says this conclusion will at first seem paradoxical. It is sufficient however to have corrected a bad thesis in mathematics to recognize how right Russell is. The candidate often is at great pains to get the first false equation; but that once obtained, it is only sport then for him to accumulate the most surprising results, some of which even may be true.

II.

We see how much richer the new logic is than the classic logic; the symbols are multiplied and allow of varied combinations *which are no longer limited in number*. Has one the right to give this extension to the meaning of the word *logic*? It would be useless to examine this question and to seek with Russell a mere quarrel about words. Grant him what he demands; but be not astonished if certain verities declared irreducible to logic in the old sense of the word find themselves now reducible to logic in the new sense—something very different.

A great number of new notions have been introduced, and these are not simply combinations of the old. Russell knows this, and not only at the beginning of the first chapter, "The Logic of Propositions," but at the beginning of the second and third, "The Logic of Classes" and "The Logic of Relations," he introduces new words that he declares indefinable.

And this is not all; he likewise introduces principles he declares indemonstrable. But these indemonstrable principles are appeals to intuition, synthetic judgments *a priori*. We regard them as intuitive when we meet

them more or less explicitly enunciated in mathematical treatises; have they changed character because the meaning of the word logic has been enlarged and we now find them in a book entitled "Treatise on Logic"? *They have not changed nature; they have only changed place.*

III.

Could these principles be considered as disguised definitions? It would then be necessary to have some way of proving that they imply no contradiction. It would be necessary to establish that, however far one followed the series of deductions, he would never be exposed to contradicting himself.

We might attempt to reason as follows: We can verify that the operations of the new logic applied to premises exempt from contradiction can only give consequences equally exempt from contradiction. If therefore after n operations we have not met contradiction, we shall not encounter it after $n+1$. Thus it is impossible that there should be a moment when contradiction *begins,* which shows we shall never meet it. Have we the right to reason in this way? No, for this would be to make use of complete induction; and *remember, we do not yet know the principle of complete induction.*

We therefore have not the right to regard these assumptions as disguised definitions and only one resource remains for us, to admit a new act of intuition for each of them. Moreover I believe this is indeed the thought of Russell and M. Couturat.

Thus each of the nine indefinable notions and of the twenty indemonstrable propositions (I believe if it were I that did the counting, I should have found some more) which are the foundation of the new logic, logic in the broad sense, presupposes a new and independent act of our intuition and (why not say it?) a veritable synthetic

judgment *a priori*. On this point all seem agreed, but what Russell claims, and what seems to me doubtful, is that after these appeals to intuition, that will be the end of it; we need make no others and can build all mathematics without the intervention of any new element.

M. Couturat often repeats that this new logic is altogether independent of the idea of number. I shall not amuse myself by counting how many numeral adjectives his exposition contains, both cardinal and ordinal, or indefinite adjectives such as several. We may cite however some examples:

"The logical product of *two* or *more* propositions is . . . .";

"All propositions are capable only of *two* values, true and false";

"The relative product of *two* relations is a relation";

"A relation exists between *two* terms," etc., etc.

Sometimes this inconvenience would not be unavoidable, but sometimes also it is essential. A relation is incomprehensible without two terms; it is impossible to have the intuition of the relation, without having at the same time that of its two terms, and without noticing they are two, because, if the relation is to be conceivable, it is necessary that there be two and only two.

V.

ARITHMETIC.

I reach what M. Couturat calls the *ordinal theory* which is the foundation of arithmetic properly so called. M. Couturat begins by stating Peano's five assumptions, which are independent, as has been proved by Peano and Padoa.

1. Zero is an integer.
2. Zero is not the successor of any integer.
3. The successor of an integer is an integer.

To this it would be proper to add,

Every integer has a successor.

4. Two integers are equal if their successors are.

The fifth assumption is the principle of complete induction.

M. Couturat considers these assumptions as disguised definitions; they constitute the definition by postulates of zero, of successor, and of integer.

But we have seen that for a definition by postulates to be acceptable we must be able to prove that it implies no contradiction.

Is this the case here? Not at all.

The demonstration cannot be made *by example.* We cannot take a part of the integers, for instance the first three, and prove they satisfy the definition.

If I take the series 0, 1, 2, I see it fulfils the assumptions 1, 2, 4, and 5; but to satisfy assumption 3, it still is necessary that 3 be an integer, and consequently that the series 0, 1, 2, 3, fulfil the assumptions; we might prove that it satisfies assumptions 1, 2, 4, 5, but assumption 3 requires besides that 4 be an integer and that the series 0, 1, 2, 3, 4, fulfil the assumptions, and so on.

It is therefore impossible to demonstrate the assumptions for certain integers without proving them for all; we must give up proof by example.

It is necessary then to take all the consequences of our assumptions and see if they contain no contradiction.

If these consequences were finite in number, this would be easy; but they are infinite in number; they are the whole of mathematics, or at least all arithmetic.

What then is to be done? Perhaps strictly we could repeat the reasoning of number III.

But as we have said, this reasoning is complete induction, and it is precisely the principle of complete induction whose justification would be the point in question.

VI.

THE LOGIC OF HILBERT.

I come now to the capital work of Hilbert which he communicated to the Congress of Mathematicians at Heidelberg, and of which a French translation by M. Pierre Boutroux appeared in *l'Enseignement mathématique,* while an English translation due to Halsted appeared in *The Monist.*[2] In this work, which contains profound thoughts, the author's aim is analogous to that of Russell, but on many points he diverges from his predecessor.

"But," he says (*Monist,* p. 340), "on attentive consideration we become aware that in the usual exposition of the laws of logic certain fundamental concepts of arithmetic are already employed, for example the concept of the aggregate, in part also the concept of number.

"We fall thus into a vicious circle and therefore to avoid paradoxes a partly simultaneous development of the laws of logic and arithmetic is requisite."

We have seen above that what Hilbert says of the principles of logic in the usual exposition, applies likewise to the logic of Russell. So for Russell logic is prior to arithmetic; for Hilbert they are "simultaneous." We shall find further on other differences still greater, but we shall point them out as we come to them. I prefer to follow step by step the development of Hilbert's thought, quoting textually the most important passages.

"Let us take as the basis of our consideration first of all a thought-thing 1 (one)" (p. 341). Notice that in so doing we in no wise imply the notion of number, because it is understood that 1 is here only a symbol and that we do not at all seek to know its meaning. "The taking of this thing together with itself respectively two, three or more times. . . ." Ah! this time it is no longer the same; if we

[2] "The Foundations of Logic and Arithmetic," *Monist* XV, 338-352.

introduce the words "two," "three," and above all "more," "several," we introduce the notion of number; and then the definition of finite whole number which we shall presently find, will come too late. Our author was too circumspect not to perceive this begging of the question. So at the end of his work he tries to proceed to a truly patching up process.

Hilbert then introduces two simple objects 1 and $=$, and considers all the combinations of these two objects, all the combinations of their combinations, etc. It goes without saying that we must forget the ordinary meaning of these two signs and not attribute any to them.

Afterwards he separates these combinations into two classes, the class of the existent and the class of the non-existent, and till further orders this separation is entirely arbitrary. Every affirmative statement tells us that a certain combination belongs to the class of the existent; every negative statement tells us that a certain combination belongs to the class of the non-existent.

IV.

Note now a difference of the highest importance. For Russell any object whatsoever, which he designates by x, is an object absolutely undetermined and about which he supposes nothing; for Hilbert it is one of the combinations formed with the symbols 1 and $=$; he could not conceive of the introduction of any thing other than combinations of objects already defined. Moreover Hilbert formulates his thought in the neatest way, and I think I must reproduce *in extenso* his statement (p. 348):

"In the assumptions the arbitraries (as equivalent for the concept 'every' and 'all' in the customary logic) represent only those thought-things and their combinations with one another, which at this stage are laid down as fundamental or are to be newly defined. Therefore in the deduc-

tion of inferences from the assumptions, the arbitraries, which occur in the assumptions, can be replaced only by such thought-things and their combinations.

"Also we must duly remember, that through the super-addition and making fundamental of a new thought-thing the preceding assumptions undergo an enlargement of their validity, and where necessary, are to be subjected to a change in conformity with the sense."

The contrast with Russell's view-point is complete. For this philosopher we may substitute for x not only objects already known but any thing.

Russell is faithful to his point of view, which is that of comprehension. He starts from the general idea of being, and enriches it more and more while restricting it, by adding new qualities. Hilbert on the contrary recognizes as possible beings only combinations of objects already known; so that (looking at only one side of his thought) we might say he takes the view-point of extension.

VIII.

Let us continue with the exposition of Hilbert's ideas. He introduces two assumptions which he states in his symbolic language but which signify, in the language of the uninitiated, that every quantity is equal to itself and that every operation performed upon two identical quantities gives identical results.

So stated, they are evident, but thus to present them would be to misrepresent Hilbert's thought. For him mathematics have to combine only pure symbols, and a true mathematician should reason upon them without preconceptions as to their meaning. So his assumptions are not for him what they are for the common people.

He considers them as representing the definition by postulates of the symbol $(=)$ heretofore void of all sig-

nification. But to justify this definition we must show that these two assumptions lead to no contradiction. For this Hilbert used the reasoning of our number III, without appearing to perceive that he is using complete induction.

IX.

The end of Hilbert's memoir is altogether enigmatic and I shall not lay stress upon it. Contradictions accumulate; we feel that the author is dimly conscious of the *petitio principii* he has committed, and that he seeks vainly to patch up the holes in his argument.

What does this mean? At the point of proving that the definition of the whole number by the assumption of complete induction implies no contradiction, Hilbert withdraws as Russell and Couturat withdrew, because the difficulty is too great.

X.

GEOMETRY.

Geometry, says M. Couturat, is a vast body of doctrine wherein the principle of complete induction does not enter. That is true in a certain measure; we cannot say it is entirely absent, but it enters very slightly. If we refer to the *Rational Geometry* of Dr. Halsted (New York, John Wiley and Sons, 1904) built up in accordance with the principles of Hilbert, we see the principle of induction enter for the first time on page 114 (unless I have made an oversight, which is quite possible).[3]

So geometry which only a few years ago seemed the domain where the reign of intuition was uncontested is to-day the realm where the logicians seem to triumph. Nothing could better measure the importance of the geometric works of Hilbert and the profound impress they have left on our conceptions.

[3] 2d. ed., 1907, p. 86; French ed. 1911, p. 97. G. B. H.

But be not deceived. What is after all the fundamental theorem of geometry? It is that the assumptions of geometry imply no contradiction, and this we can not prove without the principle of induction.

How does Hilbert demonstrate this essential point? By leaning upon analysis and through it upon arithmetic and through it upon the principle of induction.

And if ever one invents another demonstration, it will still be necessary to lean upon this principle, since the possible consequences of the assumptions, of which it is necessary to show that they are not contradictory, are infinite in number.

XI.

CONCLUSION.

Our conclusion straightway is that the principle of induction cannot be regarded as the disguised definition of the entire world.

Here are three truths: (1) The principle of complete induction; (2) Euclid's postulate; (3) The physical law according to which phosphorus melts at 44° (cited by M. Le Roy).

These are said to be three disguised definitions: the first, that of the whole number; the second, that of the straight line; the third, that of phosphorus.

I grant it for the second; I do not admit it for the other two. I must explain the reason for this apparent inconsistency.

First, we have seen that a definition is acceptable only on condition that it implies no contradiction. We have shown likewise that for the first definition this demonstration is impossible; on the other hand we have just recalled that for the second Hilbert has given a complete proof.

As to the third, evidently it implies no contradiction. Does this mean that the definition guarantees, as it should,

the existence of the object defined? We are here no longer in the mathematical sciences, but in the physical, and the word existence has no longer the same meaning. It no longer signifies absence of contradiction; it means objective existence.

You already see a first reason for the distinction I made between the three cases; there is a second. In the applications we have to make of these three concepts, do they present themselves to us as defined by these three postulates?

The possible applications of the principle of induction are innumerable; take for example one of those we have expounded above, and where it is sought to prove that an aggregate of assumptions can lead to no contradiction. For this we consider one of the series of syllogisms we may go on with in starting from these assumptions as premises. When we have finished the nth syllogism, we see we can make still another and this is the $n+1$th. Thus the number n serves to count a series of successive operations; it is a number obtainable by successive additions. This therefore is a number from which we may go back to unity by *successive subtractions*. Evidently we could not do this if we had $n=n-1$, since then by subtraction we should always obtain again the same number. So the way we have been led to consider this number n implies a definition of the finite whole number and this definition is the following: A finite whole number is that which can be obtained by successive additions; it is such that n is not equal to $n-1$.

That granted, what do we do? We show that if there has been no contradiction up to the nth syllogism, no more will there be up to the $n+1$th, and we conclude there never will be. You say: I have the right to draw this conclusion, since the whole numbers are by definition those for which a like reasoning is legitimate. But that implies

another definition of the whole number, which is as follows: A whole number is that on which we may reason by recurrence. In the particular case it is that of which we may say that, if the absence of contradiction up to the time of a syllogism of which the number is an integer carries with it the absence of contradiction up to the time of the syllogism whose number is the following integer, we need fear no contradiction for any of the syllogisms whose number is an integer.

The two definitions are not identical; they are doubtless equivalent, but only in virtue of a synthetic judgment *a priori*; we cannot pass from one to the other by a purely logical procedure. Consequently we have no right to adopt the second, after having introduced the whole number by a way that presupposes the first.

On the other hand, what happens with regard to the straight line? I have already explained this so often that I hesitate to repeat it again, and shall confine myself to a brief recapitulation of my thought. We have not, as in the preceding case, two equivalent definitions logically irreducible one to the other. We have only one expressible in words. Will it be said there is another which we feel without being able to word it, since we have the intuition of the straight line or since we represent to ourselves the straight line? First of all, we cannot represent it to ourselves in geometric space, but only in representative space, and then we can represent to ourselves just as well the objects which possess the other properties of the straight line, save that of satisfying Euclid's postulate. These objects are "the non-Euclidean straights," which from a certain point of view are not entities void of sense but circles (true circles of true space) orthogonal to a certain sphere. If, among these objects equally capable of representation, it is the first (the Euclidean straights) which we call

straights, and not the latter (the non-Euclidean straights), this is properly by definition.

And arriving finally at the third example, the definition of phosphorus, we see the true definition would be: Phosphorus is the bit of matter I see in yonder flask.

And since I am on this subject, still another word. Of the phosphorus example I said: "This proposition is a real verifiable physical law, because it means that all bodies having all the other properties of phosphorus, save its point of fusion, melt like it at 44°." And it was answered: "No, this law is not verifiable, because if it were shown that two bodies resembling phosphorus melt one at 44° and the other at 50°, it might always be said that doubtless, besides the point of fusion, there is some other unknown property by which they differ."

That was not quite what I meant to say. I should have written, "All bodies possessing such and such properties finite in number (to wit, the properties of phosphorus stated in the books on chemistry, the fusion-point excepted) melt at 44°."

And the better to make evident the difference between the case of the straight and that of phosphorus, one more remark. The straight has in nature many images more or less imperfect, of which the chief are the light rays and the rotation axis of the solid. Suppose we find the ray of light does not satisfy Euclid's postulate (for example by showing that a star has a negative parallax), what shall we do? Shall we conclude that the straight being by definition the trajectory of light does not satisfy the postulate, or on the other hand that the straight by definition satisfying the postulate, the ray of light is not straight?

Assuredly we are free to adopt the one or the other definition and consequently the one or the other conclusion; but to adopt the first would be stupid, because the ray of light probably satisfies only imperfectly not merely Euclid's

postulate but the other properties of the straight line, so that if it deviates from the Euclidean straight, it deviates no less from the rotation axis of solids which is another imperfect image of the straight line; while finally it is doubtless subject to change, so that such a line which yesterday was straight will cease to be straight to-morrow if some physical circumstance has changed.

Suppose now we find that phosphorus does not melt at 44°, but at 43.9°. Shall we conclude that phosphorus being by definition that which melts at 44°, this body that we did call phosporus is not true phosphorus, or on the other hand that phosphorus melts at 43.9°? Here again we are free to adopt the one or the other definition and consequently the one or the other conclusion; but to adopt the first would be stupid because we cannot be changing the name of a substance every time we determine a new decimal of its fusion-point.

XIII.

To sum up, Russell and Hilbert have each made a vigorous effort; they have each written a work full of original views, profound and often well warranted. These two works give us much to think about and we have much to learn from them. Among their results, some, many even, are solid and destined to live.

But to say that they have finally settled the debate between Kant and Leibnitz and ruined the Kantian theory of mathematics is evidently incorrect. I do not know whether they really believed they had done it, but if they believed so, they deceived themselves.

H. Poincaré.

Paris, France.

THE WEIRD OF LOVE AND DEATH.

"O inhabitant of Lebanon, that makest thy nest in the cedars, how gracious shalt thou be when pangs come upon thee, the pain as of a woman in travail."—Jeremiah, xxii. 23.

"Then he brought me to the door of the gate of the Lord's house, which was toward the north; and behold, there sat women weeping for Tammuz."—Ezekiel, viii. 14.

"And there followed him a great company of people, and of women, which also bewailed and lamented him.

"But Jesus turning unto them said, Daughters of Jerusalem, weep not for me, but weep for yourselves and for your children.

"For, behold, the days are coming, in which they shall say, Blessed are the barren, and the wombs that never bare, and the paps which never gave suck."—St. Luke, xxiii. 27, 28.

The author of the following verses makes no claim to be a translator, but merely an interpreter of a chapter from the Brick Bible of Babylon. He has relied upon the scholarship of others for his letter, but has sought its spirit not only beneath the text, but in the actual world of love and death. Special students of comparative religions indeed know the truth of Shakespeare's 59th sonnet:

"If there be nothing new, but that which is
Hath been before, how are our brains beguiled,
Which, laboring for invention, bear amiss
The second burthen of a former child!"

But the author has seen no other English version, in poetic form, of this oldest Semitic Gospel of the Resurrection, which, however old it be, is itself a translation, like the Greek Christian Gospels, from earlier originals.[1] Adonis has his Greek Gospels also, but only the apocryphal have come down to us, and these have found ample embodiment in Shakespeare's "Venus and Adonis."

The author has welded to the story of the Descent of Istar, two fragmentary hymns from the same literature invoking the Divine Pair, which common invocation is confirmed by the passage of Jeremiah (xxii. 18) whose Hebrew text should read, according to Prof. T. K. Cheyne: "Alas, my Brother, alas, my Sister! Alas, Adon, [Lord] alas Dodah! [Beloved Lady: a title of Istar]."

The cult of Dumu-zi-abzu (Sumerian or Akkadian "True Son of the Deep Water") whom the Hebrews alternately adored and abhorred as Tammuz, took, in its migration from the shore of the Persian Gulf to the Ægean and Sicilian coasts, only his Semitic title of "Adon." But in the course of transit the divinity first became obscured and then the human reputation. From a benign and mysterious power behind the process of spring, or a general symbol of the life principle of which winter deprives nature and death bereaves love, he became a demi-godlike huntsman and paramour of Aphrodite the goddess of beauty. Finally, in modern parlance, his epithet has dwindled to signify a pretty youth. Though coming from further east, the worship of Tammuz had its most famous seat at Aphaca (now Afka) about fifteen miles from the Phenician coast near the source of a torrent now called Ibrahim. In that ravine a crude but grand cosmic hypothesis was narrowed to a vulgar superstition. The site became a pagan Loretto or Lourdes, and developed a perverse traffic in sacred things which gave to Constantinople in the fourth Christian century the same reason or pretext for suppression that English shrines in the sixteenth century afforded Henry VIII.

The swift stream was miraculously tinged each year with the blood of the dying god whose title it then bore. It is said that the same geologic conditions stll perform the annual miracle. In the Vale of Aphaca the triumph as well as the agony of a divine victim were localized, just as later they were at Jerusalem. From Aphaca to Galilee it is but eighty miles by crow-flight, and to Nazareth less

[1] Dr. Alfred Jeremias has published the original text of the Descent of Istar with a literal German translation. An English version founded on Dr. Jeremias's translation, appeared in *The Open Court*. See Carus, "Babylonian and Hebrew Views of Man's Fate After Death," XV, p. 357.

than one hundred. Indeed, at Bethlehem (the "House of Bread") which lies seventy miles further south, the adoration of Tammuz, as an earlier fruit of the wheat than the Christian eucharist wafer, lingered in the days of St. Jerome. Though an enormous ethical distance separates the personality of Jesus from the mythical boar-chaser of Lebanon, the dogmas of Chaldea show that the traditions of the church rest on more than one foundation. The Egyptian Gospel of Osiris is another corner-stone.

The modest figure of the Virgin Mother Mary has little in common with the proud and passionate image of Istar, Ashtaroth or Astarte. Rather has the concept of her borrowed the attributes of the gracious Egyptian Isis. Istar's exchange of curses with her infernal sister, as told in clay, may nevertheless have stirred the religious feelings of her votaries among the fish-wives of Babylon. But there remains something in the grief of the divine bride for her lost bridegroom which forecasts the plastic pathos of Michelangelo's "Pieta" and is echoed in the rich harmonies of Rossini's "Stabat Mater Dolorosa."

To realm whence no echo is borne,
    to region no pioneer showeth;
To the Castle of Darkness Substantial,
    to Yesterday's shadowy shore,
Our Lady Astarte, whose beacon
    for lovers and mariners gloweth
At morning and even, descended
    and smote on the dust-laden door.

"Now open the gate unto me,
    grim warden that keepest the marches!
I would enter the Kingdom of Death!"
    cried Our Lady, the mystical Bride.
"Unless to my summons thou hearken,
    thy gate I will rend from its arches,
Setting free to outnumber the living,
    the spirits of men that have died!"

To Lady Astarte, the warden
that watcheth the entrance of Hades
Made reply: "Till I take to my mistress
thy word, prithee, Istar, forbear!"
(A feud for eternity lay
'twixt the lovely and terrible ladies,
So how should Death bid Love be welcome,
and ope to a rival her lair?)

To pitiless Queen of Irkalla
the seneschal doubtfully wended:
"Sov'reign Lady of Death, at the precinct
thy sister Astarte doth stand.
Methinks that in quest of the life-giving
water the Queen hath descended;
The bars of thy mansion are shaken
beneath her imperious hand."

To him said Queen Allat: "O warden,
as grain from the scythe of a reaper
To the Dungeon of Dust falleth Istar
imploring the water of life!
Like lip of reed that is thirsty
her need is for Tammuz the Sleeper:
But what are her sorrow and yearning
to us, or her menace of strife?

"Quoth she: 'For the hero I mourn
that hath left his wife widow'd and lonely.
I lament for the bride whose embraces
her husband hath lost and deplored;
For fate of the innocent children
whose span bore the spring-blossom only;
So lend me the water of life,
For the healing of Tammuz my Lord!'

"Yet, warden, we grant her caprice!
        Suffer Istar to enter our portal
In conformity strict to the letter
        of Death's incompassionate law.
Deprive her of every adornment
        as if she were humble and mortal;
Extinguish the glory of Istar
        that filleth the heavens with awe!"

The warden returning, threw open
        the porch of Irkalla to Istar.
"Thou art welcome to Death, O dread Lady!
        Let Ghostland rejoice in its guest!
'T is mine to conduct thee, O Queen,
        to the presence of Allat, thy sister!"
But as she stepp'd over the threshold
        he plucked from her forehead the crest.

"My crown with the crescent and star
        give back to me!" Istar besought him.
"Nay, my Princess, the code of the kingdom
        of Death even thou must obey!"
Through Second Gate when they immerged,
        as the ruler of Hades had taught him,
The warden of gloom took from Istar
        The radiant ear-rings away.

And so at each barrier passed,
        the Queen of her robe he divested,
And the necklace, the brooch and the belt
        and the bracelets he claim'd as his prey.
Relentless and brutal he was;
        When Our Lady Astarte protested,
Repeating: "Nay, Princess, the edict
        of Death even thou must obey!"

So into the hall of the hopeless,
the court of Queen Allat the Dreary,
All dishevelled, discrowned and dismantled
Our Lady Astarte he led.
Though her aspect was that of despair,
for her trials were many and weary,
Not dumb was Our Lady at sight
of the sinister Queen of the Dead.

She cursed her with formula dire,
with a torrent of bitter invective,
And she wept more in rage than in sorrow,
recounting the insults of Death.
The face of the monarch of Hades
grew scornfully sweet and reflective,
Nor uttered she one interruption
till Istar expended her breath,

Then spake with a delicate malice
these ominous words unto Istar:
"Since thou quittest the world, not a beast
of the wilderness seeketh a mate,
Nor egg hath been hatched by a fowl,
O gentle and courteous sister
Who threat'nest my realm with invasion,
but leavest thine own desolate!

"The maids of the men are unconscious,
no men to the maids make advances;
And the cradles are empty and rock'd
by the hands of no mothers to-day;
Their music the forests have lost,
the cities are stilled of their dances;
The land of the living is stagnant
since Istar to Death came away.

"My thralls thou hast sought to suborn,
    by promising thou wouldst deliver
From the dust of the grave to adore thee
    again on thy double-horned throne—
In truth, O Astarte, it seemeth,
    now Love hath discarded her quiver,
The task would be light for annexing
    the Kingdom of Life to mine own!

"Ho, Namtar! Take Istar and plague her
    with sixty-fold measure of illness!
Assail her with chastening agues
    and darken the flame of her eyes!
Let agony reign in her bosom,
    her ears have the horror of stillness!
Let clouds gather over her spirit!
    Let languor her limbs paralyse!"

Through creation there mounted a shudder
    to throne of the Father Eternal;
To the One whose dominion is screened
    by the awful illusion of space.
All nature cried out at the tyranny
    seized by the power infernal;
In conclave aghast at the rumor
    the sons of God each took his place.

"'T is June, but the leafage hath fallen;
    't is summer, but rime crusteth over
All the meads of the planets with whiteness;
    't is season for rain, but a drought
The field of ephemeral life
    with a brown desolation doth cover;
The fire of Astarte is dim;
    from the tomb cometh Tammuz not out!"

So Pápsukal, angel of light,
unto Marduk the Sun-god repeated,
Who arose and went up to his Father
and bowed in the Presence with tears.
The lord of the hours for grace
of the Infinite Spirit entreated
To call back Our Lady Astarte
from Death to her place in the spheres.

From mind of the Father Eternal
in likeness not man nor yet woman,
Did a messenger come to creation,
with countenance fair and serene.
By myriad titles invoked on the
stammering lips that are human,
Among them "Atsú-su-namír,"
and it meaneth "His Rising is seen."

"To realm whence no echo is borne,
to region no pioneer showeth;
To the Castle of Darkness Substantial;
to Yesterday's shadowy shore
Descend!" quoth the Infinite One,
"for the calm of the tempest that bloweth
From Allat the Queen of Irkalla,
the dame of the seven-fold door!

"Command her, in name of her Father,
to give from the Fount of Revival
Unto Istar her captive a draught
for the raising of Tammuz the Slain.
If pity she will not bestow
on the need of her sister and rival,
Then warn her how fragile Death's fetter
the gods Love and Life to restrain!"

More swift than the flight of a star
was the radiant herald in falling,
Through the limitless ether convey'd
on the thought of the Uttermost God.
O'er the Sea of Oblivion borne
to the Island of Silence appalling
Where hinges of Hell broke asunder
at touch of a magical rod.

Yet Allat, the Queen of the Dead,
at the luminous shape hurl'd reviling:
"Though I may not deny nor delay
my Father's unwelcome behest,
Atsú-su-namír, with the face
that is evermore hopeful and smiling,
I curse thee, who bringest His will!"
and she beat her implacable breast.

"Go, Namtar, and knock at the pillars
that hold up the base of our dwelling;
Bid the gnomes in their cavern assemble
and sit on their benches of gold;
Let Istar the water receive
that in Fount of Revival is welling,
And bring back the goddess before us;
her boon we no more may withhold."

Though grudgingly made the release,
through the seven-fold gate Lady Istar
In her strength and her beauty renewed,
from the Castle of Darkness hath gone.
No warden might check or betray,
no padlock nor bar might resist her;
With mantle and jewels restored
her figure resplendently shone.

She bore in her hand a bright chalice
for wakening Tammuz the Sleeper;
For Adonis, the First-fruits of Death,
an immortal libation she poured;
While hymns from the farthest confines
of creation grew louder and deeper,
As flowers and fishes and beasts
with mankind her arising adored:

"In Valley of Life there is growing
a tree amaranthine and shady;
From the grail of the crystal abyss
the sap of its verdure is drawn;
In heart of the earth it is rooted,
its leaves form the nest of Our Lady
Whose star in the highway of Heaven
enlight'neth the dusk and the dawn!

"Enshrined in a mystery sweet
is Adonis the Beautiful lying
On the lap of the Mother Divine
who lamented him cruelly slain.
There bloometh the garden of love,
and the flower of life is undying,
Beyond the soft veil of the temple
that hideth the deities twain!

"O Tammuz, our Lord and our Shepherd!
Miraculous Bridegroom of Istar!
Thou hast conquer'd the stronghold of Death
and thou leadest thy people like sheep!
Thou wert as the wheat in the field
that a wind of the desert doth blister,
Like tree of acacia with root
that a treacherous river doth steep!

"Our Lady, whose star in the sky
bringeth hope to the heart heavy-laden,
And whose justice on earth is a lion,
whose mercy a lamb at the breast,
O Queen of the House of the Shepherd,
O Mistress of Love ever-maiden,
May infinite joy be upon thee,
thy grief be forever at rest!"

EDWARD GILCHRIST.

SWATOW, CHINA.

CRITICISMS AND DISCUSSIONS.

THE REV. JAMES BRADLEY ON THE MOTION OF THE FIXED STARS.

(Reprinted from the *Philosophical Transactions* of 1727.)

[The theory of the relativity of time and space, which is at present uppermost in the minds of physicists, has come into the foreground mainly through the differences of measuring at large distances the time it takes light to reach the observer's eye which is further complicated by the motions of his own standpoint. This happened for the first time in the history of science in the year 1726 when Mr. Bradley discovered that the fixed stars possessed a definite and peculiar motion of their own which was due to the motion of the earth around the sun and depended on the time it takes the light to reach the earth.

This classical exposition of his experiments was published in the form of a letter sent to the *Phil. Trans.* (Vol. XXXIV, p. 637) and has naturally become quite inaccessible. There is probably only one complete file of the *Transactions* west of the Alleghanies, the fortunate possessor of which is the Chicago Public Library. Considering the rarity of this essay we deem it proper to republish it and render it accessible to our readers. We do not doubt the very way in which Mr. Bradley approaches the problem will throw much light on the principle of relativity. In fact this essay will prove sufficient to explain its far-reaching significance, the need of its invention and the limitations of its use. A consideration of the foundaton of this principle and the history of its origin will clear it of the mysticism with which its recent representations have surrounded its statements.—P. C.]

A Letter from the Reverend Mr. James Bradley, Savilian Professor of Astronomy at Oxford, and F. R. S., to Dr. Edmond Halley Astronom. Reg. &c. giving an Account of a new discovered Motion of the Fix'd Stars.

SIR,

You having been pleased to express your Satisfaction with what I had an Opportunity some time ago, of telling you in Conversation, concerning some Observations, that were making by our late worthy and ingenious Friend, the honorable *Samuel Molyneux* Esquire, and

which have since been continued and repeated by myself, in order to determine the *Parallax* of the *fixt Stars*; I shall now beg leave to lay before you a more particular Account of them.

Before I proceed to give you the History of the Observations themselves, it may be proper to let you know, that they were at first begun in hopes of verifying and confirming those, that *Dr. Hook* formerly communicated to the publick, which seemed to be attended with Circumstances that promised greater Exactness in them, than could be expected in any other, that had been made and published on the same Account. And as his Attempt was what principally gave Rise to this, so his Method in making the Observations was in some Measure that which *Mr. Molyneux* followed: For he made Choice of the same Star, and his Instrument was constructed upon almost the same Principles. But if it had not greatly exceeded the Doctor's in Exactness, we might yet have remained in great Uncertainty as to the *Parallax* of the *fixt Stars*; as you will perceive upon the Comparison of the two Experiments.

This indeed was chiefly owing to our curious Member, Mr. *George Graham*, to whom the Lovers of Astronomy are also not a little indebted for several other exact and well-contrived Instruments. The Necessity of such will scarce be disputed by those that have had any Experience in making Astronomical Observations; and the Inconsistency, which is to be met with among different Authors in their Attempts to determine small Angles, particularly the annual Parallax of the *fixt Stars*, may be a sufficient Proof of it to others. Their Disagreement indeed in this article is not now so much to be wondered at, since I doubt not, but it will appear very probable, that the Instruments commonly made use of by them, were liable to greater Errors than many times that Parallax will amount to.

The Success then of this Experiment evidently depending very much on the Accurateness of the Instrument that was principally to be taken Care of: In what Manner this was done, is not my present Purpose to tell you; but if from the Result of the Observations which I now send you, it shall be judged necessary to communicate to the Curious the Manner of making them, I may hereafter perhaps give them a particular Description, not only of Mr. *Molyneux's* Instrument but also of my own, which hath since been erected for the same Purpose and upon the like Principles, though it is somewhat different in its Construction, for a Reason you will meet with presently.

Mr. *Molyneux's Apparatus* was compleated and fitted for ob-

serving about the End of November 1725, and on the third Day of *December* following, the bright Star at the Head of *Draco* (marked ν by *Bayer*) was for the first Time observed, as it passed near the Zenith, and its Situation carefully taken with the Instrument. The like Observations were made on the 5th, 11th and 12th Days of the same Month, and there appearing no material Difference in the Place of the Star, a farther Repetition of them at this Season seemed needless, it being a Part of the Year, wherein no sensible Alteration of Parallax in this Star could be expected. It was chiefly therefore Curiosity that tempted me (being then at *Kew,* where the Instrument was fixed) to prepare for observing the Star on *December* 17th, when having adjusted the Instrument as usual, I perceived that it passed a little more Southerly this Day than when it was observed before. Not suspecting any other Cause of this Appearance, we first concluded, that it was owing to the Uncertainty of the Observations, and that either this or the foregoing were not so exact as we had before supposed; for which Reason we purposed to repeat the Observation again, in order to determine from whence this Difference proceeded; and upon doing it on *December* 20th, I found that the Star passed still more Southerly than in the former Observations. This sensible Alteration the more surprized us, in that it was the contrary way from what it would have been, had it proceeded from an annual Parallax of the Star: But being now pretty well satisfied, that it could not be entirely owing to the want of Exactness in the Observations; and having no Notion of anything else, that could cause such an apparent Motion as this in the Star; we began to think that some Change in the Materials, &c. of the Instrument itself, might have occasioned it. Under these Apprehensions we remained some time, but being at length fully convinced, by several Trials, of the great Exactness of the Instrument, and finding by the gradual Increase of the Star's Distance from the Pole, that there must be some regular Cause that produced it; we took care to examine nicely, at the Time of each Observation, how much it was: and about the Beginning of *March* 1725, the Star was found to be 20″ more Southerly than at the Time of the first Observation. It now indeed seemed to have arrived at its utmost Limit Southward, because in several Trials made about this Time, no sensible Difference was observed in its Situation. By the Middle of *April,* it appeared to be returning back again towards the North; and about the beginning of *June,* it passed at the same Distance from the Zenith as it had done in *December* when it was first observed.

From the quick Alteration of this Star's Declination about this Time (it increasing a Second in three Days) it was concluded, that it would now proceed Northward, as it before had done Southward of its present Situation; and it happened as was conjectured: for the Star continued to move Northward till *September* following, when it again became stationary, being then near 20″ more Northerly than in *June*, and no less than 39″ more Northerly than it was in *March*. From *September* the Star returned towards the South, till it arrived in *December* to the same Situation it was in at that time twelve Months, allowing for the Difference of Declination on account of the Precession of the Equinox.

This was a sufficient Proof, that the Instrument had not been the Cause of this apparent Motion of the Star, and to find one adequate to such an Effect seemed a Difficulty. A Nutation of the Earth's Axis was one of the first things that offered itself upon this Occasion, but it was soon found to be insufficient; for though it might have accounted for the change of Declination in *ν Draconis* yet it would not at the same time agree with the Phaenomena in other Stars; particularly in a small one almost opposite in right Ascension to *ν Draconis,* at about the same Distance from the North Pole of the Equator: For, though this Star seemed to move the same way, as a Nutation of the Earth's Axis would have made it, yet it changing its Declination but about half as much as *ν Draconis* in the same time (as appeared upon comparing the Observations of both made upon the same Days, at different Seasons of the Year) this plainly proved, that the apparent Motion of the Stars was not occasioned by a real Nutation, since if that had been the Cause, the Alteration in both Stars would have been near equal.

The great Regularity of the Observations left no room to doubt, but that there was some regular Cause that produced this unexpected Motion, which did not depend on the Uncertainty or Variety of the Seasons of the Year. Upon comparing the Observations with each other, it was discovered that in both the fore-mentioned Stars, the apparent Difference of Declination from the *Maxima,* was always nearly proportional to the versed Sine of the Sun's Distance from the Equinoctial Points. This was an Inducement to think, that the Cause, whatever it was, had some Relation to the Sun's Situation with respect to those Points. But not being able to frame any Hypothesis at that Time sufficient to solve all the Phænomena, and being very desirous to search a little farther into this Matter; I began to think of erecting an Instrument for myself at Wansted, that

having it always at Hand, I might with the more Ease and Certainty, enquire into the Laws of this new Motion. The Consideration likewise of being able by another Instrument, to confirm the Truth of the Observations hitherto made with *Mr. Molyneux's,* was no small Inducement to me; but the Chief of all was, the Opportunity I should thereby have of trying, in what Manner other Stars were affected by the same Cause, whatever it was. For *Mr. Molyneux's* Instrument being originally designed for observing *ν Draconis* (in order as I said before, to try whether it had any sensible Parallax) was so contrived, as to be capable of but little Alteration in its Direction, not above seven or eight Minutes of a Degree; and there being few stars within half that Distance from the Zenith of *Kew,* bright enough to be well observed, he could not, with his Instrument, thoroughly examine how this Cause affected Stars differently situated with respect to the equinoctial and solstitial Points of the Ecliptick.

These Considerations determined me; and by the Contrivance and Direction of the same ingenious Person, Mr. Graham, my Instrument was fixed up *August* 19, 1727. As I had no convenient Place where I could made use of so long a Telescope as *Mr. Molyneux's,* I contented myself with one of but little more than half the Length of his (viz. of about 12½ Feet, his being 24¼) judging from the Experience which I had already had, that this Radius would be long enough to adjust the Instrument to a sufficient Degree of Exactness, and I have no reason since to change my Opinion: for from all the Trials I have yet made, I am very well satisfied, that when it is carefully rectified, its Situation may be securely depended upon to half a Second. As the Place where my Instrument was to be hung, in some Measure determined its Radius, so did it also the Length of the Arch, or Limb, on which the Divisions were made to adjust it: For the Arch could not conveniently be extended farther, than to reach to about 6¼° on each Side my Zenith. This indeed was sufficient, since it gave me an Opportunity of making Choice of several Stars, very different both in Magnitude and Situation; there being more than two hundred inserted in the *British* Catalogue, that may be observed with it. I needed not to have extended the Limb so far, but that I was willing to take in *Capella,* the only star of the first Magnitude that comes so near my Zenith.

My instrument being fixed, I immediately began to observe such Stars as I judged most proper to give me light into the Cause of the Motion already mentioned. There was Variety enough of

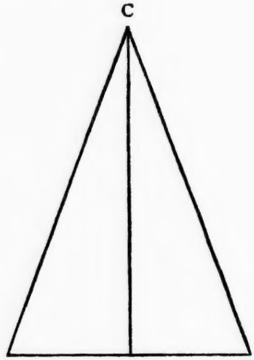
small ones; and not less than twelve, that I could observe through all the Seasons of the Year; they being bright enough to be seen in the Day-time, when nearest the Sun. I had not been long observing, before I perceived, that the Notion we had before entertained of the Stars being farthest North and South, when the Sun was about the Equinoxes, was only true of those that were near the solstitial Colure: And after I had continued my Observations a few Months, I discovered, what I then apprehended to be a general Law, observed by all the Stars, viz. That each of them became stationary, or was farthest North or South, when they passed over my Zenith at six of the Clock, either in the Morning or Evening. I perceived likewise, that whatever Situation the Stars were in with respect to the cardinal Points of the Ecliptick, the apparent motion of every one tended the same Way, when they passed my instrument about the same Hour of the Day or Night; for they all moved Southward, while they passed in the Day, and Northward in the Night; so that each was farthest North, when it came about Six of the Clock in the Evening, and farther South, when it came about Six in the Morning.

Though I have since discovered, that the *Maxima* in most of these Stars do not happen exactly when they come to my Instrument at those Hours, yet not being able at that time to prove the contrary, and supposing that they did, I endeavoured to find out what Proportion the greatest Alterations of Declination in different Stars bore to each other; it being very evident, that they did not all change their Declination equally. I have before taken notice, that it appeared from Mr. *Molyneux's* Observations, that ν Draconis altered its Declination about twice as much as the fore-mentioned small Star almost opposite to it; but examining the matter more particularly, I found that the greatest Alteration of Declination in these Stars, was at the Sine of the Latitude of each respectively. This made me suspect that there might be the like Proportion between the *Maxima* of other Stars; but finding, that the observations of some of them would not perfectly correspond with such an Hypothesis, and not knowing, whether the small Difference I met with, might not be owing to the Uncertainty and Error of the Observations, I deferred the farther examination into the Truth of this Hypothesis, till I should be furnished with a Series of Observations made in all Parts of the Year; which might enable me, not only to determine what Errors the Observations are liable to, or how far

they may safely be depended upon; but also to judge, whether there had been any sensible Change in the Parts of the Instrument itself.

Upon these Considerations, I laid aside all Thoughts at that Time about the Cause of the fore-mentioned Phaenomena, hoping that I should the easier discover it, when I was better provided with proper Means to determine more precisely what they were.

When the Year was compleated, I began to examine and compare my Observations, and having pretty well satisfied myself as to the general Laws of the Phaenomena, I then endeavoured to find out the Cause, of them. I was already convinced, that the apparent Motion of the Stars, was not owing to a Nutation of the Earth's Axis. The next Thing that offered itself, was an Alteration in the



Direction of the Plumb-line, with which the Instrument was constantly rectified; but this upon Trial proved insufficient. Then I considered what Refraction might do, but here also nothing satisfactory occurred. At last I conjectured, that all the Phaenomena hitherto mentioned, proceeded from the progressive Motion of Light and the Earth's annual Motion in its Orbit. For I perceived, that, if Light was propagated in Time, the apparent Place of a fixt Object would not be the same when the Eye is at Rest, as when it is moving in any other Direction, than that of the Line passing through the Eye and Object; and that, when the Eye is moving in different Directions, the apparent Place of the Object would be different.

I considered this Matter in the following Manner. I imagined CA to be a Ray of Light, falling perpendicularly upon the Line BD; then if the Eye is at rest at A, the Object must appear in the Direc-

tion AC, whether Light be propagated in Time or in an Instant. But if the Eye is moving from B towards A, and Light is propagated in Time, with a Velocity that is to the Velocity of the Eye, as CA to BA; then Light moving from C to A, whilst the Eye moves from B to A, that Particle of it, by which the Object will be discerned, when the Eye in its Motion comes to A, is at C when the Eye is at B. Joining the Points B, C, I supposed the Line CB, to be a Tube (inclined to the Line BD in the Angle DBC) of such a Diameter, as to admit of but one Particle of Light; then it was easy to conceive, that the Particle of Light at C (by which the object must be seen when the Eye, as it moves along, arrives at A) would pass through the Tube BC, if it is inclined to BD in the Angle DBC, and accompanies the Eye in its Motion from B to A; and that it could not come to the Eye, placed behind such a Tube, if it had any other Inclination to the Line BD. If instead of supposing CB so small a Tube, we imagine it to be the Axis of a larger; then for the same Reason, the Particle of Light at C, could not pass through that Axis, unless it is inclined to BD, in the Angle CBD. In like manner, if the Eye moved the contrary way, from D towards A, with the same Velocity; then the Tube must be inclined in the Angle BDC. Although therefore the true or real Place of an Object is perpendicular to the Line in which the Eye is moving, yet the visible Place will not be so, since that, no doubt, must be in the Direction of the Tube; but the Difference between the true and apparent Place will be (*cæteris paribus*) greater or less, according to the different Proportion between the Velocity of Light and that of the Eye. So that if we could suppose that Light was propagated in an instant, then there would be no Difference between the real and visible Place of an Object, although the Eye were in Motion, for in that case, AC being infinite with Respect to AB, the Angle ACB (the Difference between the true and visible Place) vanishes. But if Light be propagated in Time (which I presume will readily be allowed by most of the Philosophers of this Age) then it is evident from the foregoing Considerations, that there will be always a Difference between the real and visible Place of an Object, unless the Eye is moving either directly towards or from the Object. And in all Cases, the Sine of the Difference between the real and visible Place of the Object, will be to the Sine of the visible Inclination of the Object to the Line in which the Eye is moving, as the Velocity of the Eye to the Velocity of Light.

If Light moved but 1000 times faster than the Eye, and an Ob-

ject (supposed to be at an infinite Distance) was really placed perpendicularly over the Plain in which the Eye is moving, it follows from what hath been already said, that the apparent Place of such an Object will be always inclined to that Plain, in an Angle of 89° 56′½; so that it will constantly appear 3′½ from its true Place, and seem so much less inclined to the Plain, that way towards which the Eye tends. That is, if AC is to AB (or AD) as 1000 to one, the Angle ABC will be 89° 56′½, and ACB = 3′½, and BCD = 2ACB = 7′. So that according to this Supposition, the visible or apparent Place of the Object will be altered 7′, if the Direction of the Eye's Motion is at one time contrary to what it is at another.

If the Earth revolve round the Sun annually, and the Velocity of Light were to the Velocity of the Earth's Motion in its Orbit (which I will at present suppose to be a Circle) as 1000 to one; then tis easy to conceive, that a Star really placed in the very Pole of the Ecliptick, would, to an Eye carried along with the Earth, seem to change its Place continually, and (neglecting the small Difference on the Account of the Earth's diurnal Revolution on its Axis) would seem to describe a Circle round that Pole, every Way distant therefrom 3′½. So that its Longitude would be varied through all the Points of the Ecliptick every Year; but its Latitude would always remain the same. Its right Ascension would also change, and its Declination, according to the different Situation of the Sun in respect to the equinoctial Points; and its apparent Distance from the North Pole of the Equator would be 7′ less at the Autumnal, than at the vernal Equinox.

The greatest Alteration of the Place of a Star in the Pole of the Ecliptick (or which in Effect amounts to the same, the Proportion between the Velocity of Light and the Earth's Motion in its Orbit) being known; it will not be difficult to find what would be the Difference upon this Account, the Difference between the true and apparent Place of any other Star at any time; and on the contrary, the Difference between the true and apparent Place being given; the Proportion between the Velocity of Light and the Earth's Motion in its Orbit may be found.

As I only observed the apparent Difference of Declination of the Stars, I shall not now take any farther Notice in what manner such a Cause as I have here supposed would occasion an Alteration in their apparent Places in other Respects; but, supposing the Earth to move equally in a Circle, it may be gathered from what hath been already said, that a Star which is neither in the Pole nor Plain of

the Ecliptick, will seem to describe about its true Place a Figure, insensibly different from an Ellipse, whose Transverse Axis is at Right-angle to the Circle of Longitude passing through the Star's true Place, and equal to the Diameter of the little Circle described by a Star (as was before supposed) in the Pole of the Ecliptick; and whose Conjugate Axis is to its Transverse Axis, as the *Sine* of the Star's latitude to the Radius. And allowing that a Star by its apparent Motion does exactly describe such an Ellipse, it will be found, that if A be the Angle of Position (or the Angle at the Star made by two great Circles drawn from it, thro' the Poles of the Ecliptick and Equator) and B be another Angle, whose Tangent is to the Tangent of A as Radius to the Sine of the Latitude of the Star; then B will be equal to the Difference of Longitude between the Sun and the Star, when the true and apparent Declination of the Star are the same. And if the Sun's Longitude in the Ecliptick be reckoned from that Point, wherein it is when this happens; then the Difference between the true and apparent Declination of the Star (on account of the Cause I am now considering) will be always, as the Sine of the Sun's Longitude from thence. It will likewise be found, that the greatest Difference of Declination that can be between the true and apparent Place of the Star, will be to the Semi-Transverse Axis of the Ellipse (or to the Semi-diameter of the little Circle described by a Star in the Pole of the Ecliptick) as the Sine of A to the Sine of B.

If the Star hath North Latitude, the Time, when its true and apparent Declination are the same, is before the Sun comes in Conjunction with or Opposition to it, if its Longitude be in the first or last Quadrant (viz. in the ascending Semi-circle) of the Ecliptick; and after them, if in the descending Semi-circle; and it will appear nearest to the North Pole of the Equator, at the Time of that *Maximum* (or when the greatest Difference between the true and apparent Declination happens) which precedes the Sun's Conjunction with the Star.

These Particulars being sufficient for my present Purpose, I shall not detain you with the Recital of any more, or with any farther Explication of these. It may be time enough to enlarge more upon this Head, when I give a Description of the Instruments &c. if that be judged necessary to be done; and when I shall find, what I now advance, to be allowed of (as I flatter myself it will) as something more than a bare Hypothesis. I have purposely omitted some matters of no great Moment, and considerd the Earth as moving in a

Circle, and not an Ellipse, to avoid too perplexed a *Calculus,* which after all the Trouble of it would not sensibly differ from that which I make use of, especially in those Consequences which I shall at present draw from the foregoing Hypothesis.

This being premised, I shall not proceed to determine from the observations, what the real Proportion is between the Velocity of Light and the Velocity of the Earth's annual Motion in its Orbit; upon Supposition that the Phaenomena before mentioned do depend upon the Causes I have here assigned. But I must first let you know, that in all the Observations hereafter mentioned, I have made an Allowance for the Change of the Star's Declination on Account of the Precession of the Equinox, upon Supposition that the Alteration from this Cause is proportional to the Time, and regular through all the Parts of the Year. I have deduced the real annual Alteration of Declination of each Star from the Observations themselves; and I the rather choose to depend upon them in this Article, because all which I have yet made, concur to prove, that the Stars near the Equinoctial Colure, change their Declination at this time 1″½ or 2″ in a Year more than they would do if the Precession was only 50″, as is now generally supposed. I have likewise met with some small Varieties in the Declination of other Stars in different Years, which do not seem to proceed from the same Cause, particularly in those that are near the solstitial Colure, which on the contrary have altered their Declination less than they ought, if the Precession was 50″. But whether these small Alterations proceed from a regular Cause, or are occasioned by any Change in the Materials &c. of my Instrument, I am not yet able fully to determine. However, I thought it might not be amiss just to mention to you how I have endeavoured to allow for them, though the Result would have been nearly the same, if I had not considered them at all. What that is, I will shew, first from the Observations of *ν Draconis,* which was found to be 39″ more Southerly in the Beginning of *March,* than in *September.*

From what hath been premised, it will appear that the greatest Alteration of the apparent Declination of *ν Draconis,* on account of the successive Propagation of Light, would be to the Diameter of the little Circle which a Star (as was before remarked) would seem to describe about the Pole of the Ecliptick as 39″ to 40″, 4. The half of this is the Angle ACB (as represented in the Fig.) This therefore being 20″, 2, AC will be to AB, that is, the Velocity of Light to the Velocity of the Eye (which in this Case may be supposed the same as the Velocity of the Earth's annual Motion in its

Orbit) as 10210 to One, from whence it would follow, that Light moves, or is propagated as far as from the Sun to the Earth in 8′ and 12″.

It is well known, that Mr. *Romer*, who first attempted to account for an apparent Inequality in the Times of the Eclipses of *Jupiter's* Satellites, by the Hypothesis of the progressive Motion of Light, supposed that it spent about 11 Minutes of Time in its Passage from the Sun to us: but it hath since been concluded by others from the like Eclipses, that it is propagated as far in about 7 Minutes. The Velocity of Light therefore deduced from the foregoing Hypothesis, is as it were a Mean betwixt what had at different times been determined from the Eclipses of *Jupiter's* Satellites.

These different Methods of finding the Velocity of Light thus agreeing in the Result, we may reasonably conclude, not only that these *Phaenomena* are owing to the Causes to which they have been ascribed; but also, that Light is propagated (in the same Medium) with the same Velocity after it hath been reflected as before; for this will be the Consequence, if we allow that the Light of the Sun is propagated with the same Velocity, before it is reflected, as the Light of the *fixt Stars*. And I imagine this will scarce be questioned, if it can be made appear that the Velocity of the Light of all the *fixt Stars* is equal, and that their Light moves or is propagated through equal Spaces in equal Times, at all Distances from them: both which points (as I apprehend) are sufficiently proved from the apparent alteration of the Declination of Stars of different Lustre; for that is not sensibly different in such Stars as seem near together, though they appear of very different Magnitudes. And whatever their Situations are (if I proceed according to the foregoing Hypothesis) I find the same Velocity of Light from my Observations of small Stars of the fifth or sixth, as from those of the second and third Magnitude, which in all Probability are placed at very different Distances from us. The small Star, for Example, before spoken of, that is almost opposite to γ *Draconis* (being the 35th *Camelopard. Hevelii* in Mr. *Flamsteed's* Catalogue) was 19″ more Northerly about the Beginning of *March* than in *September*. Whence I conclude, according to my Hypothesis, that the Diameter of the little Circle described by a Star in the Pole of the Ecliptick would be 40″, 2.

The last Star of the great Bear's tail of the 2d Magnitude (marked η by *Bayer*) was 36″ more Southerly about the Middle of *January* than in *July*. Hence the *Maximum*, or greatest Altera-

tion of Declination of a Star in the Pole of the Ecliptick would be 40″, 4, exactly the same as was before found from the Observations of *ν Draconis.*

The Star of the 5th magnitude in the Head of *Perseus* marked *τ* by *Bayer,* was 25″ more Northerly about the End of *December* than on the 29th of *July* following. Hence the *Maximum* would be 41″. This Star is not bright enough to be seen as it passes over my Zenith about the End of *June,* when it should be according to the Hypothesis farthest South. But because I can more certainly depend upon the greatest Alteration of Declination of those Stars, which I have frequently observed about the Times when they become stationary, with respect to the Motion I am now considering; I will set down a few more Instances of such, from which you may be able to judge how near it may be possible from these Observations, to determine with what Velocity Light is propagated.

α Persei Bayero was 23″ more Northerly at the beginning of *January* than in *July.* Hence the *Maximum* would be 40″, 2. *α Cassiopeæ* was 34″ more Northerly about the End of *December* than in *June.* Hence the *Maximum* would be 40″, 8. *β Draconis* was 39″ more Northerly in the beginning of *September* than in *March*; hence the *Maximum* would be 40″, 2. *Capella* was about 16″ more Southerly in *August* than in *Feb.*; hence the *Maximum* would be about 40″. But this Star being farther from my Zenith than those I have before made use of, I cannot so well depend upon my Observations of it, as of the others; because I meet with some small Alterations of its Declination that do not seem to proceed from the Cause I am now considering.

I have compared the Observations of several other Stars, and they all conspire to prove that the *Maximum* is about 40″ or 41″. I will therefore suppose that it is 40″½ or (which amounts to the same) that Light moves, or is propagated as far as from the Sun to us in 8′ 13″. The near Agreement which I met with among my Observations induces me to think, that the *Maximum* (as I have here fixed it) cannot differ so much as a Second from the Truth, and therefore it is probable that the Time which Light spends in passing from the Sun to us, may be determined by these Observations within 5″ or 10″; which is such a degree of exactness as we can never hope to attain from the Eclipses of *Jupiter's* Satellites.

Having thus found the Maximum, or what the greatest Alternation of Declination would be in a Star placed in the Pole of the

Ecliptick, I will now deduce from it (according to the foregoing Hypothesis) the Alteration of Declination in one or two Stars, at such times as they were actually observed, in order to see how the Hypothesis will correspond with the *Phænomena* through all the Parts of the Year.

It would be too tedious to set down the whole Series of my Observations; I will therefore make Choice only of such as are most proper for my present Purpose, and will begin with those of *ν Draconis.*

This Star appeared farthest North about *September* 7th, 1727, as it ought to have done according to my Hypothesis. The following Table shews how much more Southerly the star was found to be by Observation in several Parts of the Year, and how much more Southerly it ought to be according to the Hypothesis.

1727 D.	THE DIFFERENCE OF DECLINATION BY OBSERVATION	THE DIFFERENCE OF DECLINATION BY THE HYPOTHESIS	1728 D.	THE DIFFERENCE OF DECLINATION BY OBSERVATION	THE DIFFERENCE OF DECLINATION BY THE HYPOTHESIS
Oct. 20	4½	4½	Mar. 24	37	38
Nov. 17	11½	12	April 6	36	36½
Dec. 6	17½	18½	May 6	28½	29½
Dec. 28	25	26	June 5	18½	20
1728			June 15	17½	17
Jan. 24	34	34	July 3	11½	11½
Feb. 10	38	37	Aug. 2	4	4
Mar. 7	39	39	Sept. 6	0	0

Hence it appears, that the Hypothesis corresponds with the Observations of this Star through all Parts of the Year; for the small Differences between them seem to arise from the Uncertainty of the Observations, which is occasioned (as I imagine) chiefly by the tremulous or undulating Motion of the Air, and of the Vapours in it; which causes the Stars sometimes to dance to and fro, so much that it is difficult to judge when they are exactly on the Middle of the Wire that is fixed in the common Focus of the Glasses of the Telescope.

I must confess to you, that the Agreement of the Observations

with each other, as well as with the Hypothesis, is much greater than I expected to find, before I had compared them; and it may possibly be thought to be too great, by those who have been used to Astronomical Observations, and know how difficult it is to make such as are in all respects exact. But if it would be any Satisfaction to such Persons (till I have an Opportunity of describing my Instrument and the manner of using it) I could assure them, that in above 70 Observations which I made of this Star in a Year, there is but one (and that is noted as very dubious on account of Clouds) which differs from the foregoing Hypothesis more than 2″, and this does not differ 3″.

This therefore being the Fact, I cannot but think it very probable, that the *Phænomena* proceed from the Cause I have assigned, since the foregoing Observations make it sufficiently evident, that the Effect of the real Cause, whatever it is, varies in this Star, in the same Proportion that it ought according to the Hypothesis.

But least ν *Draconis* may be thought not so proper to shew the proportion, in which the apparent alteration of Declination is increased or diminished, as those Stars which lie near the Equinoctial Colure: I will give you also the Comparison between the Hypothesis and the Observations of η *Ursæ Majoris,* that which was farthest South about the 17th Day of *January* 1728, agreable to the Hypothesis. The following Table shews how much more Northerly

1727 d.	The Difference of Declination by Observation	The Difference of Declination by the Hypothesis	1728 d.	The Difference of Declination by Observation	The Difference of Declination by the Hypothesis
Sept. 14	29½	28½	April 16	18½	18
Sept. 24	24½	25½	May 5	24½	23½
Oct. 16	19½	19½	June 5	32	31½
Nov. 11	11½	10½	June 25	35	34½
Dec. 14	4	3	July 17	36	36
1728			Aug. 2	35	35½
Feb. 17	2	3	Sept. 20	26½	26½
Mar. 21	11½	10½			

it was found by Observation in several Parts of the Year, and also what the Difference should have been according to the Hypothesis.

I find upon Examination, that the Hypothesis agrees altogether as exactly with the Observations of this Star, as the former; for in about 50 that were made of it in a Year, I do not meet with a Difference of so much as 2″, except in one, which is mark'd as doubtful on Account of the Undulation of the Air &c. And this does not differ 3″ from the Hypothesis.

The agreement between the Hypothesis and the Observations of this Star is the more to be reguarded, since it proves that the Alteration of Declination, on account of the Precession of the Equinox, is (as I before supposed) regular thro' all Parts of the Years; so far at least, as not to occasion a Difference great enough to be discovered with this Instrument. It likewise proves the other part of my former Supposition, viz. that the annual Alteration of Declination in Stars near the Equinoctial Colure, is at this Time greater than a Precession of 50″ would occasion: for this Star was 20″ more Southerly in *September* 1728, than in *September* 1727, that is, about 2″ more than it would have been, if the Precession was but 50″. But I may hereafter, perhaps, be better able to determine this Point, from my Observations of those Stars that lie near the Equinoctial Colure, at about the same Distance from the North Pole of the Equator, and nearly opposite in right Ascension.

I think it needless to give you the Comparison between the Hypothesis and the Observations of any more Stars; since the Agreement in the foregoing is a kind of Demonstration (whether it be allowed that I have discovered the real Cause of the *Phænomena* or not;) that the Hypothesis gives at least the true Law of the Variation of Declination in different Stars, with Respect to their different Situations and Aspects with the Sun. And if this is the Case, it must be granted, that the Parallax of the fixt Stars is much smaller, than hath been hitherto supposed by those who have pretended to deduce it from their Observations. I believe, that I may venture to say, that in either of the two Stars, last mentioned, it does not amount to 2″. I am of Opinion, that if it were 1″, I should have perceived it, in the great number of Observations that I made especially of *ν Draconis*; which agreeing with the Hypothesis (without allowing anything for Parallax) nearly as well when the Sun was in Conjunction with, as in Opposition to, this Star, it seems very probable that the Parallax of it is not so great as one single

Second; and Consequently that it is above 400000 times farther from us than the Sun.

There appearing therefore after all, no sensible Parallax in the fixt Stars, the *Anti-Copernicans* have still room on that Account, to object against the Motion of the Earth; and they may have (if they please) a much greater objection against the Hypothesis, by which I have endeavoured to solve the fore-mentioned *Phænomena*; by denying the progressive Motion of Light, as well as that of the Earth.

But as I do not apprehend, that either of these Postulates will be denied me by the Generality of the Astronomers and Philosophers of the present Age; so I shall not doubt of obtaining their Assent to the Consequences which I have deduced from them; if they are such as have the Approbation of so great a Judge of them as Yourself. I am

Sir, Your most Obedient
Humble Servant
J. Bradley.

POSTSCRIPT.

As to the Observations of *Dr. Hook,* I must own to you, that before Mr. *Molyneux's* Instrument was erected, I had no small opinion of their Correctness; the Length of his Telescope and the Care he pretends to have taken in making them exact, having been strong Inducements with me to think them so. And Since I have been convinced both from Mr. *Molyneux's* Observations and my own, that the Doctor's are really very far from being either exact or agreeable to the *Phænomena*; I am greatly at a loss how to account for it. I cannot well conceive that an Instrument of the Length of 36 Feet, constructed in the Manner he describes his, could have been liable to an Error of near 30″ (which was doubtless the Case) if rectified with so much Care as he represents.

The Observations of Mr. *Flamsteed* of the different Distances of the Pole Star from the Pole at different Times of the Year, which were through Mistake looked upon by some as a Proof of the annual *Parallax* of it, seem to have been made with much greater Care than those of *Dr. Hook.* For though they do not all exactly correspond with each other, yet from the whole Mr. *Flamsteed* concluded that the Star was 35″ 40″ or 45″ nearer the Pole in *December* than in *May* or *July*: and according to my Hypothesis it ought to appear 40″ nearer in *December* than in *June.* The Agreement therefore of the Observations with the Hypothesis is greater than could

reasonably be expected, considering the *Radius* of the Instrument, and the Manner in which it was constructed.

THE PRINCIPLE OF LEAST ACTION.*

REMARKS ON SOME PASSAGES IN MACH'S MECHANICS.

Ernst Mach in his *Mechanics*[1] remarks,[2] with reference to the integral variational principles of Hamilton and of least action, that *other* such principles are possible, which idea has been suggestive to myself, and, as I have obtained some results which throw light on Mach's suggestions, I will try to describe the results here in not too technical language.[3]

I.

We must first of all notice a slight historical inexactitude in Mach's treatment of the principle of least action. "Maupertuis," we are told,[4] "enunciated, in 1747, a principle which he called *'le principe de la moindre quantité d'action.'*" Maupertuis[5] laid before the Paris Academy on April 15, 1744, a memoir in which he explained the reflection and refraction of light by a hypothesis substituted for Fermat's principle of least time.[6]

Maupertuis, like a good follower of Newton, accepted the emission hypothesis of light, and, according to P. Stäckel,[7] the contra-

* Philip E. B. Jourdain, an English scholar who has devoted his life to research in the line of modern logic, mathematics and pure mechanics, submits to us some remarks on Mach's *Science of Mechanics.* He is a devoted and zealous student of Mach's works and is as familiar with them as a theologian with his Bible. Being also well acquainted with the work of Georg Cantor, Peano and Bertrand Russell he is especially fitted to explain the theoretical aspect of pure mechanics. We are confident that his lucubrations serve a good purpose and therefore deem it wise to submit them to specialists by giving them space in our columns.

[1] *Die Mechanik in ihrer Entwickelung historisch-kritisch dargestellt,* 4th ed., Leipsic, 1901, pp. 395-413; Engl. transl. by T. J. McCormack under the title *The Science of Mechanics, a Critical and Historical Account of its Development,* 3d ed., Chicago, 1907, pp. 364-380. (This translation will be referred to as *Mechanics,* and the above German edition as *Mechanik.*)

[2] *Mechanik,* pp. 399, 402, 413; *Mechanics,* pp. 368-369, 371-372, 380.

[3] Cf. note on p. 78 of my paper "On the General Equations of Mechanics," *Quarterly Journal of Mathematics,* 1904, pp. 61-79.

[4] *Mechanik,* p. 395; *Mechanics,* p. 364.

[5] Cf. *Mechanik,* pp. 484-485; *Mechanics,* pp. 454-455.

[6] *Mechanik,* pp. 454-457; *Mechanics,* pp. 422-425.

[7] *Encykl. der math. Wiss.,* IV, I, (1908), p. 49, note 125. Stäckel wrongly refers to the *Berlin Mem.,* 1745, p. 276, for Maupertuis's application of the principle of least action to the motion of light.

diction that Mach found in Maupertuis's application of the principle of least action to the motion of light is due to Mach's mistaken supposition that Maupertuis worked on the basis of the undulatory theory.

On Fermat's principle of least time and Maupertuis's principle of least action, we will quote some passages from E. T. Whittaker's lately published book, *A History of the Theories of Aether and Electricity from the Age of Descartes to the Close of the Nineteenth Century.*[8]

"Descartes's theory of light rapidly displaced the conceptions which had held sway in the Middle Ages. The validity of his explanation of refraction was, however, called in question by his fellow-countryman Pierre de Fermat (b. 1601, d. 1665), and a controversy ensued which was kept up by the Cartesians long after the death of their master. Fermat[9] eventually introduced a new fundamental law, from which he proposed to deduce the paths of rays of light. This was the celebrated *Principle of Least Time,* enunciated[10] in the form, 'Nature always acts by the shortest course.' From it the law of reflection can readily be derived, since the path described by light between a point on the incident ray and a point on the reflected ray is the shortest possible consistent with the condition of meeting the reflecting surfaces.[11] In order to obtain the law of refraction, Fermat assumed that 'the resistance of the media is different,' and applied his 'method of maxima and minima' to find the paths which would be described in the least time from a point of one medium to a point of the other. In 1661 he arrived at the solution.[12] 'The result of my work,' he writes, 'has been the most extraordinary, the most unforeseen and the happiest, that ever was; for, after having performed all the equations, multiplications, antitheses and other operations of my method, and having finally finished the problem, I have found that my principle gives exactly and precisely the same proportion for the refractions which Monsieur

[8] London and Dublin, 1910, pp. 9-11, 102-103.

[9] *Renati Descartes Epistolae, Pars tertia;* Amsterdam, 1683. The Fermat correspondence is comprised in letters xxix to xlvi.

[10] *Epist.* xlii, written at Toulouse in August, 1657, to Monsieur de la Chambre; reprinted in *Œuvres de Fermat* (ed. 1891), Vol. II, p. 354.

[11] That reflected light follows the shortest path was no new result, for it had been affirmed (and attributed to Hero of Alexandria) in the *κεφάλαια τῶν ὀπτικῶν* of Heliodorus of Larissa, a work of which several editions were published in the seventeenth century.

[12] *Epist.* xliii, written at Toulouse on Jan. 1, 1662; reprinted in *Œuvres de Fermat,* Vol. II, p. 457; Vol. I, pp. 170, 173.

Descartes has established.' His surprise was all the greater, as he had supposed light to move more slowly in dense than in rare media, whereas Descartes had (as will be evident from the demonstration given above) been obliged to make the contrary supposition.

"Although Fermat's result was correct, and, indeed, of high permanent interest, the principles from which it was derived were metaphysical rather than physical in character, and consequently were of little use for the purpose of framing a mechanical explanation of light. Descartes's theory therefore held the field until the publication in 1667[13] of the *Micrographia* of Robert Hooke (b. 1635, d. 1703), one of the founders of the Royal Society, and at one time its Secretary."

Further on, we read (p. 102): ".... the echoes of the old controversy between Descartes and Fermat about the law of refraction were awakened[14] by Pierre Louis Moreau de Maupertuis (b. 1698, d. 1759).

"It will be remembered that according to Descartes the velocity of light is greatest in dense media, while according to Fermat the propagation is swiftest in free ether. The arguments of the corpuscular theory convinced Maupertuis that on this particular point Descartes was in the right; but nevertheless he wished to retain for science the beautiful method by which Fermat had derived his result. This he now proposed to do by modifying Fermat's principle so as to make it agree with the corpuscular theory; instead of assuming that light follows the *quickest* path, he supposed that 'the path described is that by which the quantity of action is the least'; and this *action* he defined to be proportional to the sum of the spaces described, each multiplied by the velocity with which it is traversed. Thus instead of Fermat's expression

$$\int dt \text{ or } \int \frac{ds}{v}$$

(where t denotes time, v velocity, and ds an element of the path) Maupertuis introduced

$$\int v \,.\, ds$$

as the quantity which is to assume its minimum value when the path of integration is the actual path of light. Since Maupertuis's v, which denotes the velocity according to the corpuscular theory, is

[13] The *imprimatur* of Viscount Brouncker, P.R.S., is dated Nov. 23, 1664.

[14] *Mém. de l'Acad.*, 1744, pp. 417-426 [or *Œuvres de Mr. de Maupertuis*, Vol. IV, Lyons, 1756, pp. 3-18. To Maupertuis's work we will return on another occasion].

proportional to the reciprocal of Fermat's v, which denotes the velocity according to the wave-theory, the two expressions are really equivalent, and lead to the same law of refraction. Maupertuis's memoir is, however, of great interest from the point of view of dynamics; for his suggestion was subsequently developed by himself and by Euler and Lagrange into a general principle which covers the whole range of nature, so far as nature is a dynamical system."

* * *

In a memoir of 1746,[15] Maupertuis extended his hypothesis to all motions and called it the universal principle of rest and motion. By way of proving it, he derived the known laws of impact of inelastic and elastic bodies, and of the lever;[16] the motion of light having been dealt with in the memoir of 1744. It is most important to realize that, as A. Mayer[17] pointed out, Euler's discovery, made under the stimulus of the Bernoullis and published in the autumn of 1744 in an appendix to his *Methodus inveniendi,* was independent of Maupertuis, but that later on Euler's own tendency towards metaphysical speculation and the influence of Maupertuis combined to make Euler treat his principle in a less precise and more general way.

II.

Euler observed in 1744 that the differential equations of the motion of a particle are given by the simple requirement that the integral $\int v.ds$, where for the velocity v is substituted its value resulting from the principle of *vis viva,* and the integral is taken between two positions of the particle, should be a minimum. Euler

[15] "Les loix du mouvement et du repos déduites d'un principe métaphysique." *Mém. de l'Acad. de Berlin,* 1746, pp. 267-294. Voss (*Encykl. der math. Wiss.*, IV, 1, p. 95, note 256) has 1745 as the date of this memoir. This memoir was that analyzed by Mach (*Mechanik,* pp. 395-397; *Mechanics,* pp. 364-367). The analogies that exist between the motion of masses and the motion of light, which were noticed by Johann Bernoulli and by Möbius, were dealt with by Mach (*Mechanik,* pp. 402-408, 410-413, 457-459; *Mechanics,* pp. 372-380, 425-427). The principle of least action has been found very useful in optics, by Laplace, for example, in the treatment of astronomical refractions; and the mathematics of the theory of systems of rays built upon this one principle, which was the earliest work of William Rowan Hamilton, were later (in 1834 and 1835) transferred by Hamilton to the general problem of dynamics. Cf. P. Stäckel, *Encykl. der math. Wiss.*, IV, 1, 1908, pp. 489-493.

[16] In a memoir called "Loi du repos des corps" (*Mém. de l'Acad. de Paris,* 1740, pp. 170-176; *Œuvres,* Vol. IV, pp. 45-63) Maupertuis remarked that the work done when a final configuration of equilibrium is reached is generally either a maximum or a minimum (see Mach, *Mechanik,* pp. 69-75; *Mechanics,* pp. 68-73).

[17] *Geschichte des Prinsips der kleinsten Aktion,* Akademische Antrittsvorlesung, Leipsic, 1877; cf. my notes in *Ostwald's Klassiker,* No. 167, pp. 31-37.

expressly emphasized, first, that his theorem only holds if the principle of *vis viva* holds (and therefore cannot hold for motion in a resisting medium), and, secondly, that we must express v in terms of the attracting forces by quantities belonging to the orbit.[18]

Euler's work on this point was influenced adversely by his own tendency toward metaphysical speculation and Maupertuis's discovery—published some months before Euler's—of the obscure and almost theological universal "principle of the least quantity of action."[19]

III.

Lagrange[20] generalized Euler's theorem for the motion of any system of masses in the following way:

Let $m_1, m_2, m_3, \ldots$ be masses which act upon one another in any manner, and also, if we wish, move under the influence of any central forces which are proportional to any functions of the distances; let $s_1, s_2, s_3, \ldots$ be the spaces which are described by these masses in the time t, and let $v_1, v_2, v_3, \ldots$ be their velocities at the end of this time; then[21]

$$\Sigma m . \int v . ds$$

is a maximum or minimum, and thus, by the principles of the calculus of variations,

$$\Sigma m . \int (\delta v . ds + v . \delta ds) = 0. \quad \ldots\ldots\ldots\ldots\ldots (1)$$

Lagrange eliminated the terms involving δv by making use of the equation

[18] Jacobi (see below), by direct generalization of Euler's theorem, reached his theorem.

[19] The early history of the principle of least action is very fully dealt with by me in my notes at the end of *Ostwald's Klassiker der exakten Wissenschaften*, No. 167.

[20] "Application de la méthode exposée dans le mémoire précédent à la solution de différents problèmes de dynamique," *Miscellanea Taurinensia* for 1760 and 1761, Vol. II, pp. 196-298; *Œuvres de Lagrange*, Vol. I, pp. 365-468. This memoir immediately followed Lagrange's first fundamental memoir on the calculus of variations: "Essai d'une nouvelle méthode pour déterminer les maxima et les minima des formules intégrales indéfinies," *Misc. Taur.*, 1760 and 1761 [published 1762], Vol. II, pp. 173-195; *Œuvres*, Vol. I, pp. 335-362; *Ostwald's Klassiker der exakten Wissenschaften*, No. 47, pp.3-30.

In Lagrange's first publication ("Recherches sur la méthode de maximis et minimis," *Misc. Taur.* for 1759, Vol. I; *Œuvres*, Vol. I, pp. 3-20), he announced (p. 15) his intention of deriving the whole of mechanics, by means of the principle of the least quantity of action, from a method he had of investigating the maxima and minima of indefinite integral formulae.

[21] For convenience of printing, the suffixes to the Σ, m, v, and s are here omitted. Instead of the now more usual Σ Lagrange (see below) used S.

$$\Sigma m.v.\delta v = \delta U \quad \ldots\ldots\ldots\ldots\ldots\ldots (2)$$

got by varying (differentiating with δ) the equation of *vis viva.*

Thus the equation (1), in conjunction with the condition (2), supposing that all the positions at the limits of the integral are given, so that there the variations of the coordinates are zero,[22] gives the fundamental equation[23]

$$\int S dm \{ (d\frac{dx}{dt} + \Pi dt)\delta x + \ldots \} = 0, \quad \ldots\ldots\ldots (3)$$

where $$\Pi\delta x + \ldots = \delta U,$$

and S is a sign of a definite integral which refers to the masses of the system; so that, if there are a finite number of masses m_1, m_2, $m_3, \ldots$,

$$S dm = \Sigma m.$$

If there is an equation of condition $\phi = 0$ between the coordinates, the equation $\delta\phi = 0$ gives a relation between the δx's, δy's and δz's of (3); and then we can eliminate from (3) all of the variations except a certain number which is the degree of freedom of the system. If, then, we put the coefficient of every independent variation equal to zero, we obtain the necessary number of differential equations for the solution of the problem.

An important point is that, as Hölder[24] remarked, Lagrange[25] drew attention to the fact that, even when the expression for the element of work is not a complete differential, and consequently "δU" can only be regarded as an abbreviation, and not as a notation for the variation of a force-function, that the formula (2), or

$$\delta T = \delta U,$$

can be applied to get an extension of the principle of least action even to non-conservative forces. This wider form was not treated in Lagrange's later work in the *Mécanique analytique* on the principle of least action.

Thus Mach[26] is mistaken in stating that Lagrange "drew express attention to the fact that Euler's principle is applicable only in cases in which the principle of *vis viva* holds." Euler had already made this remark, and subsequently Jacobi strongly emphasized it; but Lagrange, correctly, as we now know, first drew attention to

[22] *Œuvres,* Vol. I, pp. 369-370.

[23] *Ibid.,* pp. 368, 406, 418, 435, 459.

[24] *Gött. Nachr.,* 1896, p. 136. In *Ostwald's Klassiker,* No. 167, last line on p. 39, for "Helmholtz" read "Hölder."

[25] *Œuvres,* Vol. I, pp. 384-385.

[26] *Mechank,* p. 401; *Mechanics,* p. 371.

the fact that the principle of least action, in the very general form which he gave it, does not depend for its validity on that principle of *vis viva,* which only follows from the general equations of mechanics under special conditions.

There was no mention of this extension in Lagrange's later works, and Hamilton, for example, only took from Lagrange the narrower form of the principle of least action which was given in the *Mécanique*.

* * *

Lagrange appears to have noticed that the integrand of (3), put equal to zero, is an expression of d'Alembert's principle; and, in that form, d'Alembert's principle is the fundamental formula of Lagrange's analytical mechanics,[27] and then the principle of least action became, for Lagrange, merely a result of the laws of mechanics, to be got by the integration of the simpler equation.

However, in the early memoir Lagrange had concluded from his generalized principle of least action nearly all the great results which later, in his *Mécanique,* he derived in another way; and so Jacobi[28] remarked that Lagrange's principle became the mother of our whole analytical mechanics.[29]

[27] D'Alembert's principle in combination with the principle of virtual displacements appeared in the above variational form for the first time in a prize essay of 1764 of Lagrange's on the libration of the moon (*Œuvres,* Vol. VI, pp. 5-61); and then, more fully, in a memoir of 1780 (*Œuvres,* Vol. V., pp. 5-122).

The various editions of Lagrange's *Mécanique* are: *Mécanique analitique,* Paris 1788, I vol.; second, greatly enlarged edition, *Mécanique analytique,* Paris, Vol. I, 1811, Vol. II (posthumous), 1815; third edition, with notes by J. Bertrand, 2 vols., Paris, 1853 and 1855; fourth edition, after the third, but with additional notes by G. Darboux, in *Œuvres de Lagrange,* Vols. XI, and XII, Paris, 1888 and 1889.

[28] See *Compt. Rend.,* Vol. V, 1837, pp. 61-67 (*Ges. Werke,* Vol. IV, pp. 129-136); *Vorlesungen über Dynamik, gehalten an der Universität zu Königsberg im Wintersemester* 1842-1843 *und nach einem von C. W. Borchardt ausgearbeiteten Hefte herausgegeben von A. Clebsch,* Berlin, 1866, p. 2 (2d ed., revised by E. Lottner, in Jacobi's *Ges. Werke, Supplementband*). Cf. A. Mayer, *Geschichte des Prinzips der kleinsten Action,* Leipsic, 1877, p. 26 (on Mayer's errors see my notes in *Ostwald's Klassiker,* No. 167).

In this early memoir the problems treated by Lagrange were: the motion of one body attracted by many fixed central forces; general problem of many attracting masses under any other forces; the finding of the orbits of two attracting bodies with respect to a third; a body in a plane under forces and drawing two other bodies by threads; a thread fixed at one end and charged with as many heavy bodies as wished; an inextensible thread, all the points being under any forces; the same problem with an extensible and elastic thread; motion of a body of any figure animated by any forces; laws of the motion of non-elastic and elastic fluids.

[29] However, Lagrange's method of multipliers (Mach, *Mechanik,* pp. 499-500; *Mechanics,* p. 471) appeared first in the *Méchanique analitique* of 1788.

After the publication of the *Mécanique,* the principle of least action fell into the background of interest until Hamilton, in 1834, showed that this principle had also a totally different title to our consideration. The only really important contribution to the exceedingly interesting questions that rise *à propos* of the principle of least action was an almost entirely neglected one made by Olinde Rodrigues in 1816.

IV.

In Lagrange's derivation, the variation of $v(=ds/dt)$ is not carried out, but the terms $m.v.\delta v$ are eliminated by the variational equation obtained from the principle of *vis viva.* Thus it is not necessary to decide whether t must be varied or not, whether we must put

$$\delta v = \frac{d\delta s}{dt} - \frac{ds}{dt}\frac{d\delta t}{dt} \quad \text{or} \quad \delta v = \frac{d\delta s}{dt}.$$

It is almost beyond doubt that Lagrange would have maintained that the independent variable t was to be varied;[30] but Rodrigues was the first explicitly to say that, in this case, t must be varied.

Lagrange had worked with a *space* integral $\int\Sigma m.v.ds$, and had only remarked, in a short addition to the section on the principle of least action, made in the second edition of the *Mécanique,* that the above space integral transforms into the *time*-integral $\int 2T.dt$, where $2T$ is the *vis viva* (or, as we now say, double the kinetic energy) of the system.[31] But Lagrange did not actually carry out the calculation of the variation of this time-integral; this was done by Rodrigues.[32] Rodrigues, as E. J. Routh[33] did later and apparently independently, to find the variation of $\int T.dt$ under the condition $T = U + \text{const.}$ for the variation, so that $\delta T - \delta U = 0$, multiplied the left-hand side of this last equation of condition by an undetermined factor, integrated it, added it to the variation of $\int T.dt$, put all equal to zero, and then determined the factor.

[30] Cf. *Œuvres,* Vol. I, pp. 337, 345; and *Ostwald's Klassiker,* No. 167, p. 56.

[31] See *Ostwald's Klassiker,* No. 167, p. 11.

[32] *Correspondance sur l'Ecole polytech.,* Vol. III, 1816, pp. 159-162; German translation, with notes on some errors of Rodrigues, in *Ostwald's Klassiker,* No. 167, pp. 12-15, 41-42, 49-55.

[33] First in *An Elementary Treatise on the Dynamics of a System of Rigid Bodies,* 3d ed., London, 1877, pp. 305-312, 560-562. This passage coincides in essentials with *The Advanced Part* [Part II] *of a Treatise on the Dynamics of a System of Rigid Bodies,* 6th ed., London, 1905, pp. 301-309.

V.

The question as to whether the independent variable should be varied in the calculus of variations is of great importance to our conception of this calculus. According to Mach, [34] the first satisfactory explanation of the meaning of the process of variation used in this calculus was given by J. H. Jellett.[35] The value of the function $y = \phi(x)$ can vary by an (infinitesimal) increment dx of the independent variable, when we obtain the *differential*

$$dy = \phi(x + dx) - \phi(x),$$

or by the varying of the form ϕ of the function without x varying, so that $\phi(x)$ becomes

$$\phi_1(x) = \phi(x) + \epsilon\psi(x),$$

where ψ is an arbitrary function and ϵ is, for the definition of an *infinitesimal* variation, an infinitely small positive number. Then the *variation* of y is defined by

$$\delta y = \phi_1(x) - \phi(x).$$

Thus, if we keep, as is convenient, the term "variation" to denote alterations of value brought about by alteration of the form alone of the function, we see that the independent variable is unaffected by our process of variation. On the other hand, Lagrange, as we have seen, held that the independent variable also was to be affected by the δ of the calculus of variations. Indeed, his claim that his method was more general than that of Euler rested partly on this ground. But other mathematicians appear mostly to have accepted that conception of a variation which Euler gave in a later memoir on Lagrange's method, that a "variation" of a function is brought about by a change in value of the constants occurring in that function. Thus, Jacobi, in his *Vorlesungen über Dynamik*,[36] stated that the variations δq of the generalized coordinates q contain merely the changes in value of the q's which arise from changes in value of the arbitrary constants occurring in the q's. Accordingly, he maintained[37] that the independent variable is not to be "varied," so that $\delta t = 0$.[38]

[34] *Mechanik*, pp. 468-474; *Mechanics*, pp. 437-443.

[35] *An Elementary Treatise on the Calculus of Variations*, Dublin, 1850, pp. 1, 5-6. Cf. A. Kneser, *Lehrbuch der Variationsrechnung*, Brunswick, 1900, pp. 1-2.

[36] *Werke, Supplementband*, p. 145.

[37] *Ibid.*, pp. 50, 59, 146, 149.

[38] Cf. similar views on the nature of a "variation" with Euler, Lagrange, Lacroix, G. W. Strauch, M. Ohm, Cauchy, and Stegmann in I. Todhunter's

So Jacobi, in his *Vorlesungen über Dynamik*,[39] stated that, in the action integral $\int\Sigma m.v.ds$, the time must be eliminated by the principle of *vis viva*, and all be reduced to space-elements. This, as Mayer remarked in his tract of 1877, was required by Euler in the case considered by him. Thus Jacobi's[40] formulation of the principle of least action was: If two positions of the system are given (that is to say, if we know the values which, for $x=a$ and $x=b$, the remaining $3n-1$ coordinates receive), and we extend the integral

$$\int \sqrt{2(\mathrm{U}+h)}\ \sqrt{\Sigma m\ .\ ds^2}$$

to the whole path of the system from the first position to the second, then its value is a minimum for the actual path as compared with all possible (consistent with the conditions, if there be any, of the system) paths.[41]

Mayer, in his tract of 1877,[42] accepted Jacobi's view that $\delta t=0$ and consequently that, by means of the principle of *vis viva*, we must reduce all the quantities in the integrand to quantities which refer to the path of the system; and that the theorem of least action without this condition is quite meaningless. Since Lagrange did not eliminate the time, Mayer[43] concluded that Lagrange's theorem was meaningless, and what Lagrange really meant by his theorem was what is known as Hamilton's principle. This view had been previously maintained by M. Ostrogradski.[44]

But, in a memoir of 1886 on the general theorems of the calculus of variations which correspond to the two forms of the principle of least action in dynamics, Mayer[45] remarked, on the variation

work *A History of the Progress of the Calculus of Variations During the Nineteenth Century*, Cambridge and London, 1861, pp. 2, 8, 11, 13, 17-20, 31, 377, 378, 402, 413, 480-481.

[39] *Werke, Supplementband*, p. 44; *Ostwald's Klassiker*, No. 167, p. 17 (on pp. 16-26 is a reprint of Jacobi's sixth and part of his seventh lecture, which relate to the principle of least action).

[40] *Werke, Supplementband*, p. 45; *Ostwald's Klassiker*, No. 167, p. 18 (cf. the note on p. 55).

[41] On the limitations to the minimum-condition, which were pointed out by Jacobi (cf. Mach, *Mechanik*, p. 401; *Mechanics*, p. 371) see *Werke, Suppl.*, pp. 45-49; *Klassiker*, No. 167, pp. 18-22, 58.

[42] See p. 24, and *Klassiker*, No. 167, p. 57.

[43] *Op. cit.*, p. 27.

[44] *Klassiker*, No. 167, pp. 57-58.

[45] "Die beiden allgemeinen Sätze der Variationsrechnung, welche den beiden Formen des Prinzips der kleinsten Aktion in der Dynamik entsprechen," *Berichte der math.-phys. Classe der Kön. Sächs. Ges. der Wiss. zu Leipzig*, Sitzung am 14. November 1886, Vol. XXXVIII, pp. 343-355. The first person correctly to show the importance of Rodrigues's memoir was Th. Sloudsky

of t with Rodrigues: "Now, from the point of view of dynamics, in which we only permit variations from the instantaneous position of the system under consideration, that is so very unusual that I did not think at all of this possibility in my earlier work. But as soon as we neglect a purely dynamical signification (*Deutung*), and vary, not only the coordinates, but also the time, immediately that point which always caused the greatest doubts in Lagrange's derivation becomes clear. It is explained, namely, how the equation of *vis viva*, if it is prescribed as an equation of condition, can yet leave the variations of the coordinates quite unlimited,[46] and we see then that Jacobi's assertion that we must necessarily eliminate the time from the action-integral by means of the theorem of *vis viva* is not so; that, besides Jacobi's principle, there is a second, equally justified form of the principle of least action; and that it is this second form, and not Hamilton's principle inaccurately formulated, which Lagrange proved correctly, though certainly not with his usual clearness.

We may here remark that Routh,[47] from 1877 onwards and apparently independently of Rodrigues, also varied t, "by the fundamental theorem in the calculus of variations," and derived the principle of least action as Rodrigues did.

If t is to be varied, we must regard it, according to the conception of a "variation" derived from Jellett, as a function of another variable, θ, so that $\delta\theta = 0$ but δt is not zero in general. This was done explicitly by Helmholtz[48] in 1887.

Helmholtz also stated the view that Hamilton's principle is a form of Lagrange's principle. The grounds for this view are, as I showed in 1908,[49] more clearly evidenced in an identity established by Réthy under certain restrictions.

VI.

We have dealt with the question as to the relation of the principle of least action to Hamilton's principle, and we have seen how Lagrange, by working with a form which only contained the time through the velocities, and in which the variations of the velocities

(1866); Bertrand, in his notes on Lagrange's *Mécanique*, mentioned Rodrigues, but put $\delta(dq/dt) = d\delta q/dt$.

[46] Cf. *Klassiker*, No. 167, pp. 43-44.

[47] Cf. *ibid.*, pp. 50-51.

[48] "Zur Geschichte des Prinzips der kleinsten Aktion," *Sitzungsber. der Berliner Akad.*, Sitzung vom 10. März 1887, pp. 225-236; *Wiss. Abh.*, Vol. III, pp. 249-263.

[49] *Math. Ann.*, Vol. LXV, pp. 514-516.

could be at once eliminated by means of the varied equation of *vis viva,* allowed it to remain doubtful whether t was to be varied in the principle of least action, or not. We have seen how this question has given rise to discussions and misunderstandings which are connected with the principle of the calculus of variations, in the works of Rodrigues, Jacobi, Ostrogradski, Routh, Mayer, Sloudsky, Bertrand, Helmholtz, and Réthy. We have seen, finally, that Lagrange had attained to a very general formulation of the principle of least action, in which the equation of *vis viva* does not hold, a force-function does not exist, and the equations of condition may depend explicitly on the time. Thus Lagrange's principle is far more general than Jacobi's.

Of late years, the occurrence of differential and non-integrable equations among the equations of condition of a problem has assumed great importance. This happens in certain cases of rolling motion, and systems with such equations of condition were called by Hertz *non-holonomous.* The question arises as to whether the principle of least action and Hamilton's principle can be so formulated as to apply to non-holonomous systems. We shall see that Otto Hölder first succeeded in formulating extended forms of both principles which were completely equivalent to d'Alembert's principle. There were, of course, several points not dealt with by Hölder on which it was essential to be quite clear. Thus, the process of "variation" used by Hölder was not always the one to which we are accustomed in the calculus of variations, and the transformation of the principles from rectangular coordinates—which alone were used by Hölder—to more general coordinates gives rise to interesting questions. However, it seems to me that we have now reached a certain degree of finality in all these subjects, and we will now present the researches whose object was to extend the principles, in their proper order, and, where necessary, comment on them.

VII.

The question as to the extent of the variational principles begins with the publication, in 1894, of Heinrich Hertz's posthumous *Prinzipien der Mechanik.*[50] "The application of Hamilton's prin-

[50] *Gesammelte Werke von Heinrich Hertz,* Vol. III, *Die Prinzipien der Mechanik in neuem Zusammenhange dargestellt* (edited by Ph. Lenard, with a preface by H. von Helmholtz), Leipsic, 1894; English translation by D. E. Jones and J. T. Walley under the title *The Principles of Mechanics,* London, 1899.

ciple," said Hertz,[51] "to a material system does not exclude fixed connections between the coordinates chosen, but it requires that these connections can be exposed mathematically by means of finite equations between the coordinates; it does not permit of such connections as can be expressed only by differential equations. But nature itself appears not simply to exclude connections of the latter kind; for they occur if, for example, three-dimensional bodies roll upon one another without slipping."

Hertz[52] called a material system *holonomous* if between possible positions all thinkable continuous passages are also possible. The name was chosen to indicate that such a system is subject to integral (ὅλος) laws (νόμος), while material systems in general are subject only to differential laws. If the differential equations of condition of a material system can all be integrated, the coordinates of every possible position must satisfy the finite equations. The differences between the coordinates of two neighboring positions therefore satisfy an equal number of homogeneous linear differential equations, and, since these latter cannot contradict the given differential equations (in equal number) of the system, they satisfy the latter too. Thus the displacement between any two possible positions is a possible displacement, and thus the system is holonomous. Inversely, if the system is holonomous, its differential equations of condition allow an equal number of finite or integral equations between the coordinates themselves.

VIII.

Here we may digress to remark that the fact that cases of rolling motion give rise to equations of condition which are not integrable was observed by Routh, Ferrers (1873), and C. Neumann (1888).[53] The usual form of Lagrange's equations then fails. Of the extensions, what I have called, in the paper just quoted, "Routh's form" is the most important form for our present purposes. It involves Lagrange's multipliers, and is the only form of equation valid for non-holonomous systems which can be got directly by development of one of the integral variational principles. In deducing equations of motion from, say, Hamilton's principle,

[51] *Werke,* Vol. III, pp. 22-25; *Principles,* pp. 19-21.

[52] *Werke,* Vol. III, articles 123, 132, and 133 (pp. 91, 95, and 96); *Principles,* pp. 80, 84-85.

[53] Cf. the note on p. 63 of my paper "On the General Equations of Mechanics," *Quart. Journ. of Math.,* 1904, pp. 61-79. Cf. the bibliography in P. Appell's little book on *Les mouvements de roulement en dynamique,* Paris, 1899.

we so to speak divide the material system into a holonomous and a non-holonomous part. Suppose there are $3n$ rectangular coordinates of the system, k finite equations of condition between these coordinates and the time, and l non-integrable equations of condition. We form our integral for a system with $3n-k$ degrees of freedom and then eliminate the l superfluous coordinates by Lagrange's method.

IX.

An important paper on the differential equations of mechanics was written by A. Voss[54] in 1884 and published in 1885. In this paper, the equations of condition were used in their differential form, and were not assumed to be integrable, although the problems of rolling motion which caused such equations to be considered were not mentioned. The part which especially concerns us here is where Voss uses Hamilton's principle for the introduction of more general coordinates. He says[55] that, with non-integrable equations of condition, "the transformation can no longer be reduced to a problem of variations properly so called, but the property of the system of differential equations of condition of being a complete one forms the necessary and sufficient condition for this."

X.

Hertz decided that his own fundamental law[56] holds both for holonomous and non-holonomous systems, and that from this law result the principle of least action[57] and Hamilton's principle[58] only under a limitation to holonomous systems. But this contradicts the general conviction[59] that Hamilton's principle is merely a transformation of d'Alembert's principle, and that the latter holds generally, and is equivalent to Hertz's law.[60] Thus arose the questions as to whether the usual derivation of Hamilton's principle from that of d'Alembert requires any limiting supposition. This question was the origin of the researches of Otho Hölder.[61] The very kernel of

[54] "Ueber die Differentialgleichungen der Mechanik," *Math. Ann.*, Vol. XXV, 1885, pp. 258-286.

[55] *Ibid.*, pp. 263-264.

[56] *Werke*, Vol. III, art. 309, p. 162; *Principles*, p. 144.

[57] *Werke*, Vol. III, arts. 347-356, pp. 174-176; *Principles*, pp. 155-157.

[58] *Werke*, Vol. III, arts. 358-362, p. 177; *Principles*, pp. 158-159.

[59] See, for example, Mach, *Mechanik*, pp. 413-414; *Mechanics*, p. 381.

[60] *Werke*, Vol. III, art. 394, p. 186; *Principles*, p. 166.

[61] "Ueber die Principien von Hamilton und Maupertuis," *Nachr. von der königl. Ges. der Wiss. zu Göttingen, Math. phys. Klasse*, 1896, pp. 122-157.

Hölder's work is his conception of the "variation of the motion of a system;" this it was which allowed him to give such a wide extension to the principles of least action and of Hamilton, so that the reply to the above question is: If d'Alembert's principle holds generally, so also must that of Hamilton, in its completest form; but if we choose Hertz's view that the varied path be a possible one, we get the limitation denoted by him. Hölder's conception of a varied motion is, then, paradoxical in so far that this "motion" need not be a possible one,—need not satisfy the equations of condition. It is, in Hölder's own words, only a mathematical auxiliary conception.

With Hertz, Hölder understood by "the position of a system" the totality of the positions of the material points of the system; the motion consists in a continuous sequence of positions of the system, which are passed through in a definite way with the time. To vary this original motion, we first give every system-position a small displacement, so that a new continuous sequence of positions arises. If the original sequence gives one position twice, we have two positions covering one another which can naturally be displaced in different manners. The starting position A and the final position B are to be fixed, and we refer each position on the varied path to one on the actual path. This correspondence is necessary in order that we may put the variation of an integral taken along the original path equal to the integral of the varied elements. We coordinate the identical initial positions to one another, and similarly with the two final positions.

If we imagine both the actual[62] and the fictitious motion to begin simultaneously at A, then the systems need not arrive at B simultaneously. In this case the corresponding positions on the two paths cannot all be passed simultaneously, and if the passage from an actual position to the corresponding position on the fictitious path be denoted by δ, so that, if the position P is actually reached at the time t, the corresponding position $P+\delta P$ is reached at the time $t+\delta t$ and $\delta(dt)=d(\delta t)$.

Now, in the most general manner of variation of the motion, we can still choose the velocity at each point of the varied path. This must be infinitely little different from the velocity at the corresponding position of the actual path, but is otherwise arbitrary.

Hölder then found the expression for δT in rectangular coordi-

[62] Thus it is assumed that the mechanical problem has one solution and one only.

nates, t and dt being affected by the δ-process, integrated the identity for δT from t_0 to t_1 (the times when the system, in the actual motion, is at A and B respectively), and integrated by parts. Thus, two parts are obtained: one integrated, which vanishes, since the variations of the coordinates at A and B vanish; and the other unintegrated, and we see by d'Alembert's principle, that the integrand of the last integral can be put equal to δU where, as before, "δU" only denotes the variation of a force function U in special cases—provided that the variations of the coordinates represent *virtual* displacements of the system.[63] Thus Hölder obtained the result that, where the δ-process is a process of giving every position P between A and B a virtual displacement to P+δP, and the aggregate of positions P+δP is conceived as a fictitious path, then the equation

$$\int\{2T.d\delta t + (\delta T + \delta U)dt\} = 0, \quad \ldots\ldots\ldots\ldots (4)$$

where the integral is to be taken between the limits t_0 and t_1, is equivalent to d'Alembert's principle.

We cannot too strongly emphasize the nature of this varied path of the system. It is not necessarily a path that the system, however constrained, could take; that is to say, the connections of the system might have to be distorted from point to point. The displacement δP must be virtual at the instant t, but the position $P+\delta P$ is "reached" by the system, supposed to "move" on a fictitious path in a perhaps impossible way, at the, in general different, time $t+\delta t$. In fact, the fictitious path is only a possible one, of course under new constraints, if the equations of the condition are independent of the time, and the system is holonomous.

This fictitious motion is a useful conception because it enables us to see exactly why Hertz, for example, rather naturally limited the scope of the principle of least action and Hamilton's principle to holonomous systems; and also it allows us to formulate these principles in a perfectly general manner. That the conception of a "variation" is not that of the calculus of variations did not escape Hölder. "At the first glance," he wrote,[64] "the conception is perhaps peculiar, and it has been already said to me that I have no problem of variation properly so called. But that does not concern me. I am only concerned with giving a clear signification to the variations of the coordinates and the time which at the same time is such that

[63] That is to say, displacements consistent with the equations of condition and possible, at the instant considered. Cf., for example, Mach, *Mechanik*, p. 58; *Mechanics*, pp. 49, 56.

[64] In a letter to me of Jan. 15, 1904; cf. *Quart. Journ. of Math.*, 1904, p. 75, last note.

the principles hold as generally as is possible." In conformity with this, Hölder spoke of an "altered" (*abgeänderte*) instead of a "varied" motion.

In the above general principle, we can, without detracting from the equivalence to d'Alembert's principle, specialize the variations. Two ways at once suggest themselves:

(1) We may determine that corresponding positions are to be passed at the same instant, so that $\delta t = 0$, then (4) becomes a generalized Hamilton's principle;

(2) We may determine the velocity at each point of the varied path by fixing that $\delta T = \delta U$, the variation of the time being, of course, not zero; that is to say, using a more restricted phraseology for this wider case, the total energy is constant in a variation; then (4) gives the principle of least action in its most extended form.

XI.

There is one rather important point upon which Hölder only touched very briefly. I mean the introduction of other more general coordinates into the development of equations of motion from the above principles. Voss attempted to do this in 1900, but, as I have shown, [65] he used a method previously used by Routh and Réthy, which preserved the strictly variational character of the δ-process used even when the equations of condition depend explicitly on the time. Thus Voss unintentionally abandoned Hölder's δ-process. The application of Hölder's process to the formulation of the principles in general coordinates was first carried out by myself in the above cited paper of 1904, and more clearly in a paper of 1908.[66] Mathematically speaking, this formulation is not quite so simple as some might suppose; but here we are only concerned with the advantages of Hölder's δ-process over the strictly variational process in the formulation of the principle of least action and Hamilton's principle. The abandonment of the strict conception of a variation may appear to be a disadvantage. But surely this is compensated by greater simplicity; while, in my case, when we come to deal with non-holonomous systems we must abandon this strict conception, as was pointed out—we have seen above—by Voss in 1884 and by others later in somewhat different forms.[67] Further, unless the

[65] *Math. Ann.*, Vol. LXV, 1908, pp. 517-525.

[66] *Math. Ann.*, Vol. LXV, 1908, pp. 525-527.

[67] C. Neumann (1888), Hertz (1894), Hölder (1896), and Appell (1898); see also Boltzmann, *Vorlesungen über die Prinzipe der Mechanik*, Teil II, Leipsic, 1904, pp. 30-34.

equations of condition do not contain the time explicitly, the form of Réthy and Voss requires a condition holding for δt at the limits of integration, whereas in Hölder's generalized principle of least action no such condition is required.

XII.

As we have said at the beginning, Mach has stated, with reference to the principles of least action and Hamilton, that *other* such principles are possible. In this connection there are two investigations to which we must refer. The first was by Voss[68] in 1901, and was inspired by Hölder's work. Voss remarked that if not only the coordinates, but also the time is varied in the most general manner, δt can always be determined subsequently so that if we put the variation of the integral of *any* function of the coordinates and velocities equal to zero, we get the equation of motion. The second was an attempt by myself[69] to solve the problem suggested by Mach, by determining *all the possible integral variational principles.* For this purpose I inquired what was the most general form of the integrand in order that the principle obtained hence should be equivalent to Routh's extension of Lagrange's equations. The result was to find that Hölder's principle (4) was the most general of its kind, and, as Hölder had remarked, his principle may be specialized into Hamilton's principle or the principle of least action. These two principles are, in fact, two special cases out of the manifold of the principles equivalent to d'Alembert's principle and derivable from (4) by determining δt generally in all possible ways.

But there is another aspect of the matter. We have taken Lagrange's equations, or rather Routh's extension of them, as fundamental. But there are other forms of the equations of mechanics involving other quantities than Lagrange's T and U, and which sometimes present advantages over Lagrange's.[70] From these other equations we can derive[71] other variational principles not contained in Hölder's form (4), but since the functions in the integrand now involve differential coefficients with respect to t of the second

[68] "Bemerkungen über die Prinzipien der Mechanik," *Sitzber. der math.-phys. Klasse der k. Bayer. Akad. der Wiss. zu München*, Vol. XXXI, 1901, pp. 167-182, especially pp. 171-175; *Encykl. der math. Wiss.*, IV, 1, 1901, p. 94.

[69] *Quart. Journ. of Math.*, 1904, pp. 76-78.

[70] Cf. my paper on "Alternative Forms of the Equations of Mechanics," in the *Quart. Journ. of Math.*, 1905, pp. 284-296.

[71] Cf. *Ibid.*, pp. 290-295.

order, we must determine the varied path so that not only the variations δq but also the differentials $d\delta q$ of these variations vanish at the limits of integration. Analogous conditions as to the paths arise, if the integrand contains higher differential coefficients than the second.

XIII.

A curious result,[72] by the way, is that if we vary the integral of action $\int 2T.dt$, so that δx means, as with Hölder, a virtual displacement of x, and vary t, we get exactly the same result as if we had not varied t either in T or in dt: the extra terms we get from varying t happen to cancel one another. Hence the faulty derivation, which we sometimes see, of Hamilton's principle from the principle of least action leads to correct results. This derivation is: Since $\delta T = \delta U$, we have

$$\delta\int 2T.dt = \int(\delta T + \delta T)dt = \int(\delta T + \delta U)dt = \delta\int(T+U)dt.$$

It should be noticed that the extra terms above referred to cancel even if the equations of condition contain the time explicitly. Further, we have seen that the identification maintained by Helmholtz and Réthy of Hamilton's principle with the principle of least action depended on the equations of condition not containing the time explicitly; and that the other identifications were based on misunderstandings. Finally, we have seen how in Hölder's other work, the true relation of the principles became clear, and how, at the same time, the principle became generalized.

XIV.

This sketch of the development and gradual generalization of a small part of the theory of mechanics gives us food for meditation. It seems to be necessary, in order that it may be possible to state the principles in question quite generally, to make use of a paradoxical conception—the conception of a generalized, fictitious "motion." It would be easy to say that the principles are, by the laws of logic, valid only under certain conditions; hence the paradox when we attempt to widen those conditions. But the paradox is not logical; it is merely verbal. We speak of a fictitious "path" and "motion" merely for the sake of picturesqueness: a mathematician no more means to imply the existence, in a mystical region of thought, of an impossible and fictitious path or motion, than he means to imply anything more than striking analogies of expression when he speaks,

[72] *Quart. Journ. of Math.*, 1904, pp. 78-79.

in analytical geometry, of "imaginary intersections" or "circular points at infinity." No philosopher wishes to confute a mathematician because, in his technical language, the mathematician may assert that some "real" numbers are not "rational."

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NOTES ON THE CONSTRUCTION OF MAGIC SQUARES

OF ORDERS IN WHICH n IS OF THE GENERAL FORM $4p+2$.

It is well known that magic squares of the above orders, i. e., $6^2, 10^2, 14^2, 18^2$, etc., cannot be made perfectly pandiagonal and ornate with the natural series of numbers.

Dr. C. Planck has however pointed out that this disability is purely arithmetical, seeing that these magics can be readily constructed as perfect and ornate as any others with a properly selected series of numbers.

In all of these squares n is of the general form $4p+2$, but they can be divided into two classes:

Class I. Where n is of the form $8p-2$, as 6^2, 14^2, 22^2 etc.

Class II. Where n is of the form $8p+2$, as 10^2, 18^2, 26^2 etc.

The series for all magics of Class I may be derived by making a square of the natural series 1 to $(n+1)^2$ and discarding the numbers in the middle row and column.

Thus, for a 6^2 magic the series will be:

1	2	3	—	5	6	7
8	9	10	—	12	13	14
15	16	17	—	19	20	21
—	—	—	—	—	—	—
29	30	31	—	33	34	35
36	37	38	—	40	41	42
43	44	45	—	47	48	49

The series for all magics of Class II may be made by writing a square of the natural numbers 1 to $(n+3)^2$ and discarding the numbers in the *three* middle rows and columns. The series for a 10^2 magic, for example, will be:

1	2	3	4	5	.	.	.	9	10	11	12	13
14	15	16	17	18	.	.	.	22	23	24	25	26
27	28	29	30	31	.	.	.	35	36	37	38	39
40	41	42	43	44	.	.	.	48	49	50	51	52
53	54	55	56	57	.	.	.	61	62	63	64	65
—	—	—	—	—	.	.	.	—	—	—	—	—
—	—	—	—	—	.	.	.	—	—	—	—	—
—	—	—	—	—	.	.	.	—	—	—	—	—
105	106	107	108	109	.	.	.	113	114	115	116	117
118	119	120	121	122	.	.	.	126	127	128	129	130
131	132	133	134	135	.	.	.	139	140	141	142	143
144	145	146	147	148	.	.	.	152	153	154	155	156
157	158	159	160	161	.	.	.	165	166	167	168	169

By using series as above described, pandiagonal magics with double-ply properties, or associated magics may be readily made either by the La Hireian method with magic rectangles, or by the path method as developed by Dr. C. Planck.

Referring now to the La Hireian method and using the 6^2 magic as a first example, the rectangles required for making the two auxiliary squares will necessarily be 2×3, and the numbers used therein will be those commonly employed for squares of the seventh order, i. e., $(6+1)^2$, with the middle numbers omitted thus:

1	2	3	—	5	6	7
0	7	14	—	28	35	42

It may be shown that a magic rectangle having an odd number of cells in one side, and an even number of cells in the other side is impossible with consecutive numbers, but with a series made as above it can be constructed without any difficulty, as shown in Figs. 1 and 2.

Two auxiliary squares may now be made by filling them with their respective rectangles. If this is done without forethought, a plain pandiagonal magic of the sixth order may result, but if attention is given to ornate qualities in the two auxiliaries, these features will naturally be carried into the final square. For example,

by the arrangement of rectangles shown in Figs. 3 and 4 both auxiliaries are made magic in their six rows, six columns and twelve

7	2	3
1	6	5

Fig. 1.

42	7	14
0	35	28

Fig. 2.

7	2	3	7	2	3
1	6	5	1	6	5
7	2	3	7	2	3
1	6	5	1	6	5
7	2	3	7	2	3
1	6	5	1	6	5

Fig. 3.

0	42	0	42	0	42
35	7	35	7	35	7
28	14	28	14	28	14
0	42	0	42	0	42
35	7	35	7	35	7
28	14	28	14	28	14

Fig. 4.

7	44	3	49	2	45
36	13	40	8	41	12
35	16	31	21	30	17
1	48	5	43	6	47
42	9	38	14	37	10
29	20	33	15	34	19

Fig. 5.

7	2	3	3	2	7
1	6	5	5	6	1
7	2	3	3	2	7
1	6	5	5	6	1
7	2	3	3	2	7
1	6	5	5	6	1

Fig. 6.

0	42	0	42	0	42
35	7	35	7	35	7
28	14	28	14	28	14
28	14	28	14	28	14
35	7	35	7	35	7
0	42	0	42	0	42

Fig. 7.

diagonals, and they are also 4-ply and 9-ply. Their complementary couplets are also harmoniously connected throughout in steps of

3, 3. These ornate features are therefore transmitted into the finished 6^2 magic shown in Fig. 5. If it is desired to make this square associated, that is with its complementary couplets evenly balanced around its center, it is only necessary to introduce the feature of association into the two auxiliary squares by a rearrangement of their magic rectangles as shown in Figs. 6, 7 and 8, the last figure being a pandiagonal associated magic.

7	44	3	45	2	49
36	13	40	12	41	8
35	16	31	17	30	21
29	20	33	19	34	15
42	9	38	10	37	14
1	48	5	47	6	43

Fig. 8.

The next larger square of Class I is 14^2, and it can be made with the natural series 1 to $(14+1)^2$ arranged in a square, discarding, as before, all the numbers in the central row and column.

The rectangles for this square will necessarily be 2×7 and the numbers written therein will be those ordinarily used for a square of the fifteenth order, $(14+1)^2$, with the middle numbers omitted, thus:

1	2	3	4	5	6	7	—	9	10	11	12	13	14	15
0	15	30	45	60	75	90	—	120	135	150	165	180	195	210

15	2	3	12	11	6	7
1	14	13	4	5	10	9

Fig. 9.

210	15	30	165	150	75	90
0	195	180	45	60	135	120

Fig. 10.

Simple forms of magic rectangles for the auxiliaries are shown in Figs. 9 and 10, but many other arrangements of the couplets will work equally well.

The smallest magic of Class II is 10^2, the series for which is given below. The rectangles used for filling the two auxilliaries of this square are 2×5, and they can be made with the numbers which

would be commonly used for a square of the thirteenth order $(10+3)^2$ omitting the three middle numbers in each row thus:

1	2	3	4	5	.	.	.	9	10	11	12	13
0	13	26	39	52	.	.	.	104	117	130	143	156

Figs. 11 and 12 show these two rectangles with a simple arrangement of the numbers. The two auxiliaries and the finished 10^2

13	2	11	4	5
1	12	3	10	9

Fig. 11.

156	13	130	39	52
0	143	26	117	104

Fig. 12.

magic are given in Figs. 13, 14 and 15. Fig. 15 is magic in its ten rows, ten columns and twenty diagonals. It is also 4-ply and 25-ply. Like the 6^2 magic, this square can also be associated by changing the disposition of the magic rectangles in the auxiliaries.

13	2	11	4	5	13	2	11	4	5
1	12	3	10	9	1	12	3	10	9
13	2	11	4	5	13	2	11	4	5
1	12	3	10	9	1	12	3	10	9
13	2	11	4	5	13	2	11	4	5
1	12	3	10	9	1	12	3	10	9
13	2	11	4	5	13	2	11	4	5
1	12	3	10	9	1	12	3	10	9
13	2	11	4	5	13	2	11	4	5
1	12	3	10	9	1	12	3	10	9

Fig. 13.

The above examples will suffice to explain the general construction of these squares by the La Hireian method with magic rectangles. It may however be stated that although the series previously described for use in building these squares include the lower numerical values, there are other series of higher numbers which will produce equivalent magic results.

0	156	0	156	0	156	0	156	0	156
143	13	143	13	143	13	143	13	143	13
26	130	26	130	26	130	26	130	26	130
117	39	117	39	117	39	117	39	117	39
104	52	104	52	104	52	104	52	104	52
0	156	0	156	0	156	0	156	0	156
143	13	143	13	143	13	143	13	143	13
26	130	26	130	26	130	26	130	26	130
117	39	117	39	117	39	117	39	117	39
104	52	104	52	104	52	104	52	104	52

Fig. 14.

13	158	11	160	5	169	2	167	4	161
144	25	146	23	152	14	155	16	153	22
39	132	37	134	31	143	28	141	30	135
118	51	120	49	126	40	129	42	127	48
117	54	115	56	109	65	106	63	108	57
1	168	3	166	9	157	12	159	10	165
156	15	154	17	148	26	145	24	147	18
27	142	29	140	35	131	38	133	36	139
130	41	128	43	122	52	119	50	121	44
105	64	107	62	113	53	116	55	114	61

Fig. 15.

The following table illustrates another rule covering the selection of numbers for all magic squares of these orders.

ORDER OF SQUARE	NATURAL SERIES	DISCARDING NUMBERS IN
6th	1 to $(6+1)^2$	the middle row and column.
10th	1 to $(10+3)^2$	the 3 middle rows and columns.
14th	1 to $(14+5)^2$	the 5 middle rows and columns.
18th	1 to $(18+7)^2$	the 7 middle rows and columns.
22nd	1 to $(22+9)^2$	the 9 middle rows and columns.
26th	1 to $(26+11)^2$	the 11 middle rows and columns.
		and so forth.

These figures show that this rule is equivalent to taking the numbers of the natural series $\left(\frac{3n-4}{2}\right)^2$ and omitting the central $\frac{n-4}{2}$ rows and columns. In comparing the above with the rules previously given, for which we are indebted to Dr. C. Planck, it will be seen that in cases of magics larger than 10^2 it involves the use of unnecessarily large numbers.

The numerical values of the ply properties of these squares are naturally governed by the dimensions of the magic rectangles used in their construction. Thus the rectangle of the 6^2 magic (Fig. 5) is 2×3, and this square is 2^2-ply and 3^2-ply. The rectangle of the 10^2 magic being 2×5, the square may be made 2^2-ply and 5^2-ply, and so forth.

The formation of these squares by the Path method which has been so ably developed by Dr. C. Planck,[1] may now be considered. The first step is to rearrange the numbers of the given series in such a cyclic order or sequence, that each number being written consecutively into the square by a well defined rule or path, the resulting magic will be identical with that made by the La Hireian method, or equivalent thereto in magic qualities. Starting, as before, with the 6^2 magic, the proper sequence of the first six numbers is found in what may be termed the "continuous diagonal" of its magic rectangle. Referring to Fig. 1, this sequence is seen to be 1, 2, 5, 7, 6, 3, but it is obvious that there may be as many different sequences as there are variations in the magic rectangles.

The complete series given on page 304 must now be rearranged

[1] "The Theory of Path Nasiks," by C. Planck, M.A., M.R.C.S., published by A. T. Lawrence, Rugby, England.

in its *lines and columns* in accordance with the numerical sequence of the first six numbers as above indicated. To make this arrangement quite clear, the series given on p. 304 is reproduced in Fig. 16, the numbers written in circles outside the square showing the numerical order of lines and columns under rearrangement. Fig. 17 shows the complete series in new cyclic order, and to construct a square directly therefrom, it is only necessary to write these numbers consecutively along the proper paths. Since the square will be pandiagonal *it may be commenced anywhere,* so in the present example we will place 1 in the fourth cell from the top in the first column, and will use the paths followed in Fig. 5 so as to reproduce that square. The paths may be written $\begin{vmatrix} 3, & 2 \\ 4, & 3 \end{vmatrix}$ and since we can always write

	1	2	6	3	5	4
1	1	2	3	5	6	7
2	8	9	10	12	13	14
6	15	16	17	19	20	21
3	29	30	31	33	34	35
5	36	37	38	40	41	42
4	43	44	45	47	48	49

Fig. 16.

	1	2	3	4	5	6
1	1	2	5	7	6	3
2	8	9	12	14	13	10
3	29	30	33	35	34	31
4	43	44	47	49	48	45
5	36	37	40	42	41	38
6	15	16	19	21	20	17

Fig. 17.

$-(n-a)$ instead of a, we may write this $\begin{vmatrix} 3, & 2 \\ -2, & 3 \end{vmatrix}$. This only means that the numbers in the first column of Fig. 17 (which may be termed the *leading numbers*) are to be placed in order along the path (3, 2), as in the numbers enclosed in circles in Fig. 5; and then starting from each cell thus occupied, the remaining five numbers in each of the six rows of Fig. 17 are to be written along the path (−2, 3). It will be seen that this is equivalent to writing the successive rows of Fig. 17 intact along the path (−2, 3), or (3, −2) and using a "break-step" (1, −1), as in Fig. 18 where the first break-step is shown with an arrow. The break-step is always given by summing up the coordinates; thus, the paths here being $\begin{vmatrix} 3, & 2 \\ -2, & 3 \end{vmatrix}$, by summing the columns we get (1, 5), that is (1, −1). The resulting square is, of course, identical with Fig. 5.

As previously stated, this square being pandiagonal, it may be

commenced in any of its thirty-six cells, and by using the same methods as before, different aspects of Fig. 5 will be produced. Also, since by this method complementary pairs are always sepa-

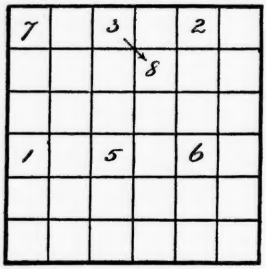

Fig. 18.

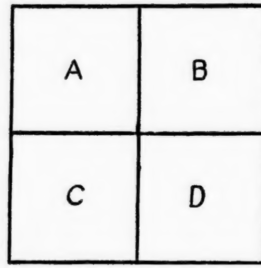

Fig. 19.

rated by a step $(n/2, n/2)$, any of the thirty-six squares thus formed may be made associated by the method described in *The Monist*, Vol. XX, No. 3, page 443, under the heading "Magic Squares by

1	2	3	4	9	13	12	11	10	5
14	15	16	17	22	26	25	24	23	18
27	28	29	30	35	39	38	37	36	31
40	41	42	43	48	52	51	50	49	44
105	106	107	108	113	117	116	115	114	109
157	158	159	160	165	169	168	167	166	161
144	145	146	147	152	156	155	154	153	148
131	132	133	134	139	143	142	141	140	135
118	119	120	121	126	130	129	128	127	122
53	54	55	56	61	65	64	63	62	57

Fig. 20.

Complementary Differences,"[2] viz., Divide the square into four quarters as shown in Fig. 19; leave A untouched, reflect B, invert C and

[2] Errata in this article: p. 440, footnote, and p. 443, fourth line from top of page, instead of "for all orders $=4n+2$" read "for orders wherein n is of the general form $4p+2$."—Page 44, last line, for "order $8n$" read "this class."

reflect and invert D. For this concise and elegant method of changing the relative positions of the complementary couplets in a square we are indebted to Dr. Planck.

The next square in order is 10^2. The series of numbers used is given on page 305 and their rearrangement in proper cyclic order

13	160	2	161	11	169	4	158	5	157
27	140	38	139	29	131	36	142	35	133
117	56	106	57	115	65	108	54	109	63
144	23	155	22	146	14	153	25	152	16
130	43	119	44	128	52	121	41	122	50
1	166	12	165	3	157	10	168	9	159
39	134	28	135	37	143	30	132	31	141
105	62	116	61	107	53	114	64	113	55
156	17	145	18	154	26	147	15	148	24
118	49	129	48	120	40	127	51	126	42

Fig. 21.

for direct entry may be found as before in the continuous diagonal of its magic rectangle. The sequence shown in Fig. 11 is 1, 2, 3, 4, 9, 13, 12, 11, 10, 5, and the complete rearrangement of the series in accordance therewith is given in Fig. 20. Various 10^2 magics may be made by using this series with different paths. The paths $\begin{vmatrix} 5, & 4 \\ -4, & 5 \end{vmatrix}$

21	2	3	4	17	16	15	8	13
1	20	19	18	5	6	7	14	9

Fig. 22.

will produce Fig. 15, and $\begin{vmatrix} 5, & 2 \\ 2, & 5 \end{vmatrix}$ will make Fig. 21, which is equivalent to Fig. 15 in its ornate features.

These squares and all similarly constructed larger ones of these orders may be changed to the form of association wherein the complementary couplets are evenly balanced around the center of the

square, by the method previously explained. It will be unnecessary to prolong the present article by giving any examples of larger squares of this class, but the simple forms of magic rectangles for

23	2	21	4	19	6	17	8	9	10	13
1	22	3	20	5	18	7	16	15	14	11

Fig. 23.

29	2	27	4	25	6	23	8	9	20	11	18	13
1	28	3	26	5	24	7	22	21	10	19	12	17

Fig. 24.

18^2 and 22^2 and 26^2 magics, shown in Figs. 22, 23 and 24, may be of some assistance to those who desire to devote further study to these interesting squares.[3]

W. S. ANDREWS.
L. S. FRIERSON.

A NEW THEORY OF INVENTION.

A Russian engineer, P. K. von Engelmeyer of Moscow (Petersburger Chaussée 42), has published a little book on invention and its significance in our industrial life under the title *Der Dreiakt* (Berlin, Carl Heymann's Verlag) in which he claims that man is not only a political being (ζῷον πολιτικόν) as Aristotle claims, but also and mainly a technical being (ζῷον τεχνικόν), and he means it in the same sense in which Franklin called man a "tool-making animal."[1]

Mr. Engelmeyer defines technique as the art of reproducing artificially or intentionally certain desired phenomena (p. 17) and he calls attention to the fact that we are surrounded by the products of invention. Our clothes, the light and heat in our houses, our mode of traveling, in short, all that is called culture and civilization has

[3] More generally, if p, q are relative primes, the square of order pq will be magic on its pq rows, pq columns and $2pq$ diagonals, and at the same time p^2-ply and q-^{2}ply, if it be constructed with the paths $\begin{vmatrix} p, & q \\ q, & p \end{vmatrix}$, and the period be taken from the continuous diagonal of the magic rectangle $p \times q$. The limitations are dictated by the magic rectangle. Evidently p and q must both be > 1, and consecutive numbers must fail if the order is $\equiv 2 \pmod{4}$; in all other cases consecutive numbers will suffice. C. P.

[1] See the author's *The Philosophy of the Tool*, p. 1.

been invented at various times. Some inventions have been made by conscious endeavor, others by accident.

Our author distinguishes four characteristics of invention: (1) its artificial nature—man interferes with natural conditions and introduces a human element into them: (2) teleology—inventions must be designed, they must serve a purpose; (3) surprise, by which word our author means that they must be something new or original; we do not call invention what is merely an application of former experience; (4) unity—every invention is a kind of a system, an organic whole, and the members must be integral parts of a new entirety. Discovery is somewhat different from invention, but there is a domain which belongs practically to both invention and discovery. Newton's law of gravitation is a discovery, but mathematical formulas are both.

Mr. Engelmeyer quotes Goethe approvingly when he says: "man does not experience or enjoy without at the same time being productive," thus implying that invention is an indispensable element in human existence. There are three fields of human activity. When man devotes his efforts to purposes of utility, the result is called invention; when his efforts are devoted to cognition, the result is called discovery; when this result serves esthetical pleasure it is called a work of art. Just as all three domains are ultimately one, so there must be but one theory of invention which our author calls by the Greek name "Heurology," and in so far as it expresses this union he calls it an act of three, or in German *Dreiakt*.

This theory of the *Dreiakt* is the subject of the main part of the book, and the author has consulted the patent laws of different nations for details and illustrations. From the standpoint of his conception he distinguishes between the product and the method of an invention; the former is the effect accomplished, the latter is the arrangement of parts, the combination of substances in definite proportions, the way in which substances are treated to change their nature. The patent lawyer must consider the principle which comprises the effect together with the way in which it is produced. Examples are furnished by the sewing machine, the bicycle, hydraulic systems, aeronautics, fire arms, chemical inventions, cement, explosives, photography, Bessemer steel, etc.

The concluding chapter of the book is devoted to the application of the *Dreiakt* to patent laws and technical instruction. The universality of the principle of the *Dreiakt* finds appreciation in the proposition that the human will itself is a *Dreiakt*. Our auther

gives credit to O. Schanze who has published his views on the same subject under the title *Beiträge zur Lehre von der Patentfähigkeit*, fascicle 2, pages 243-255 (Berlin, Siemens, 1904). He uses the term *Dreiakt* in a slightly different sense and speaks of three fundamental energies: (1) intention or will, (2) reflection or knowledge, (3) practical skill. These characterize every act of creation as a *Dreiakt*, (1) the aim which constitutes the teleology of the work, (2) the plan or design which logically determinates the work and (3) its execution. Schanze applies them to practical problems, especially to these three: *a*, Who among several collaborators is the author of the invention and who merely an assistant; *b*, how far in its application is an invention entitled to protection by patent; and *c*, at what state of completion does an invention acquire the right to be patented.

P. C.

BOOK REVIEWS AND NOTES.

The Periplus of the Erythraean Sea. By *Wilfred H. Schoff*. New York: Longmans, Green and Company, 1912. Pp. 323. Price $2.00 net.

"Periplus" means circumnavigation and may be freely translated "log book" or "description of a sea voyage." There are several antique books which bear the same title, and the present work refers to that body of water which in modern times is known as the Indian Ocean, together with the Red Sea and the Persian Gulf. This record describes the voyage from place to place of an ancient merchant vessel and is of great interest in the history of trading. The book itself is not long. It contains only 28 pages of English text, but the translation has been made with great care. Very full notes explain the terms used, the merchandise traded and the historical connections, and these cover pages 50 and 282; tables are appended listing articles of trade and rulers mentioned and dates variously assigned to the original, a map indicates the ports touched at and helps the readers to understand the geography of our travelers. The book is furnished with a very thorough topical index covering thirty two-columned pages. The work is creditable to the spirit of the Commercial Museum of Philadelphia, which has brought it out. W. P. Wilson, the director of the Philadelphia museums, says in his foreword:

"The Periplus of the Erythræan Sea is the first record of organized trading with the nations of the East, in vessels built and commanded by subjects of the Western world. The notes add great interest, giving as they do an exhaustive survey of the international trade between the great empires of Rome, Parthia, India and China, together with a collection of facts touching the early trade of a number of other countries of much interest." κ

The Individual and Reality. By *Edward Douglas Fawcett*. New York: Longmans, Green & Co., 1909. Pp. 449.

The author writes as one having authority. He considers himself a free lance, since he is independent of any school of philosophy or religion, and therefore "free to ignore all traditions and conventions and go straight to reality in the search for truth." The present volume is intended to supersede a former one to which he refers as "my *Riddle*." This former work was read with enormous satisfaction by the late Prof. William James, and the fact that this same thinker considers his book "as a great and powerful agency in the spreading of truth" is regarded by the author as sufficient justification for its appearance. Mr. Fawcett credits the source of his thinking to the anti-

Hegelian thought of Schelling and Schopenhauer, being allied to the former's "immemorial being" and Bain's doctrine of relativity. He has not read Bergson with whom his results in part seem to agree. In this later work he has abandoned the monadology of his former production, replacing it with a new form of idealism. Some of the novelties of the former work are here retained. Part I is an introduction to metaphysics; Part II treats of the individual and his universe, appearances, and the individual in his relation to the organism, nature as a whole, and to himself. Part III deals with ultimate questions, such as the ground of appearance, the evolution of nature and individuals, birth, death, destiny and God. *ρ*

PROTESTANT THOUGHT BEFORE KANT. By *Arthur Cushman M'Giffert*. New York: Charles Scribner's Sons, 1911. Pp. 261. Price, 75 cents net.

In this volume dedicated to Adolf Harnack, the author's teacher and friend, Professor M'Giffert, of Union Theological Seminary of New York, traces the development of Protestantism from the time of its early workings in medieval Christianity to that of the great Königsberg philosopher. This includes a discussion of the leaders of the Reformation in all countries, Zwingli, Melanchthon, Calvin; also the radical anabaptists and socianians. The Protestant phase of scholasticism is discussed, and pietism in Germany, England and New England. The book closes with a chapter on rationalism in its various phases as found in England, France, Germany and America. Professor M'Giffert's book is the sixth volume in a series entitled "Studies in Theology" which are intended as aids to interpretation in biblical criticism, primarily for the use of ministers and theological students, but still the needs of the general reader are kept in view so that the works shall not become too technical. *ρ*

UEBER KLASSIKER UND PHILOSOPHEN DER NEUZEIT. By *Julius Rupp*. Leipsic: Eckardt, 1910. Pp. 796 . Price 6 m.

This is the third volume of Rupp's collected works which are to appear in twelve volumes, and have been edited by P. C. Elsenhans. Each volume has a separate introduction by the editor. The present one on the classicists and modern philosophers contains three different collections of Rupp's essays. The first set, from Lessing to Hegel, are on subjects relating to Lessing, Kant, Herder, Spinoza, Schiller, Fichte and Schleiermacher. The second collection, bearing the general title "Contemporary Philosophy," discusses subjects relating to natural science, controversies about the soul, macrocosm, and microcosm, the relations of soul to body, philosophy, theology, religion of the spirit; and devotes special chapters to Emerson, Gioberti and Alexander Bain. This volume contains also Rupp's "Sketches of a Thinker." *ρ*

THE PHENOMENOLOGY OF MIND. By *G. W. F. Hegel*. Tr. by *J. B. Baillie*. 2 vols. London: Sonnenschein, 1910. Pp. 823. Price 21 s. net.

This translation of Hegel's *Phenomenology* is one number of Sonnenschein's "Library of Philosophy" edited by Dr. J. H. Muirhead. The object of the series is to familiarize English readers with results of modern philo-

sophical thought, admitting that in this respect Germany has far excelled England. The editor's purpose, however, besides bringing German philosophy to English thinkers, is to furnish a systematized philosophical library in which English philosophy will receive the consideration due it, as its significance has been largely ignored by the German schools. *ρ*

DAVID HUME, HANS LIV OG HANS FILOSOFI. Af *Anton Thomsen*. Copenhagen: Nordiske Forfatteres Forlag, 1911. Pp. 458. Price 1.65 kr.

Professor Anton Thomsen of the University of Copenhagen is preparing an extremely comprehensive work on the subject of this great English philosopher. The first volume appeared during last winter, and after a few introductory pages calling attention to the bicentenary of David Hume's birth, takes up in its first book the philosopher's life and works, and in the second book his epistemology and psychology. The philosophical critique is made with due reference to the contemporary thinkers of all lands in connection with the philosophy of past centuries. *ρ*

L'EVOLUTION DE LA MÉMOIRE. Par *Henri Piéron*. Paris: Flammarion, 1910. Pp. 360. Price 3.50 fr.

This book treats of the extent of the domain of the memory and the relations of the phenomena of inorganic memory to those of psychic memory; of the forms which memory assumes in all the steps of the evolution of animals and the continuity of the series when passing from brute creation to man; of the aspects and limitations of human memory, the cause of its difficulties and its probable future. The discussion of these points is based on the collection of facts actually established by objective psychology, human and comparative.

The conclusion drawn is both pessimistic and optimistic: pessimistic, because it sees no chance for the memory of men regarded individually to increase in capacity, and because the utilization of the traces left by collective memory (i. e., presented by published material) seems likely to become more and more difficult; optimistic, in that the conservation of many recollections will become less and less necessary in the progress of scientific classification which will make possible the substitution of the knowledge of a small number of general laws for that of a large number of particular facts. *ρ*

DAS KÜNFTIGE JAHRHUNDERT DER PSYCHOLOGIE. Von *G. Heymans*. Aus dem Niederländischen übersetzt von *H. Pol*. Leipsic: Barth, 1911. Pp. 52. Price 1 m. 20.

Prof. G. Heymans, retiring rector of the Groningen University, has published his oration in the translation of Mr. H. Pol, the German teacher of the same university. It bears the title "The Future Century of Psychology," and insists that while progress is rapid in other branches the development of psychology ought not to be neglected because it is more important than our progress in inventions. The main subjects of psychology refer to the nature of our own self, our relation to others and toward the ultimate foundation of the world. He finds that much is to be done and much has been neglected

in former ages. In opposition to the common view that competition and war are necessary, that mankind is bad at the core, he quotes Frederick the Great as having said of a prominent educator: "Ah, mon cher Sulzer, vous ne connaissez pas assez cette maudite race, à laquelle nous appartenons!" In opposition to the current view he expects that the future will more and more restrict competition and war, and criticizes the idea that they are necessary for the amelioration of the race; that if the principle were reasonable cattle breeders and hunters might just as well introduce it into the artificial methods of producing higher and better breeds, but what would we think of a hunter who would make his hounds quarrel about a piece of meat in a fierce fight in which half of them would lose their lives, and this simply for the amelioration of the race? He ends his oration by quoting the words of a mystic thinker, "I trust that all will yet be good." *κ*

In Hamburg, the place of the first monistic congress, a free religious society has been founded which proposes to do a propaganda for a rational world conception. Their aims are through religious devotion to cultivate the true, the good and the beautiful. In politics they favor separation of church from state and of school from church. Their secretary is Bruno Heyer, and their treasurer Adolf Dunkel. *κ*

The well-known activity of the Leipsic publishing house of Dürr is seen by constant additional contributions to its *Philosophische Bibliothek,* and the value of its productions is attested by the height to which the number of its editions reaches. Among its 1910 publications, besides the seventh edition of Baensch's translation of Spinoza's Ethics, we have an edition by Johannes Schubert of Wilhelm von Humboldt's selected philosophical writings and the Definitions of Christian Wolff collected by Julius Baumann for the purpose of serving as collateral reading in the study of Kant. The centennial of the Berlin University has been celebrated by this enterprising house by a volume introduced by Edouard Spranger and containing the addresses of Fichte, Schleiermacher and Steffins on "The Nature of the University," written or delivered at the time of its opening. A second edition of Dr. Otto Apelt's German translation of Plato's *Theaetetus* bears the date of 1911, and purports to be an entirely new translation of the dialogue. (*Baruch de Spinoza, Ethik,* übers. von Otto Baensch; *Fichte, Schleiermacher, Steffens über das Wesen der Universität,* her. von Edouard Spranger; *Wilhelm von Humboldts ausgewählte philosophische Schriften,* her. von. Johannes Schubert; *Wolffsche Begriffsbestimmungen,* her. von Julius Baumann; *Platons Dialog Theätet,* übers. von Dr. Otto Apelt.) *ρ*

The scientific publishing house of A. Hermann and Son, at Paris, are publishing a French translation of the sixth German edition of Prof. W. Nernst's large work on theoretical chemistry. It is translated by A. Corvisy, under the title *Traité de chimie générale.* The first part is issued this year, dealing with the general properties of bodies and atoms and molecules. *ρ*